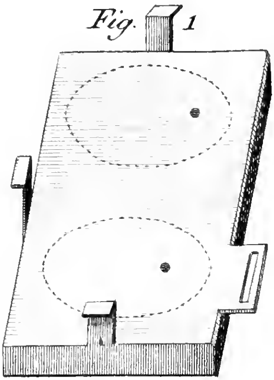
Fig. 1



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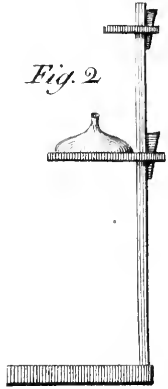

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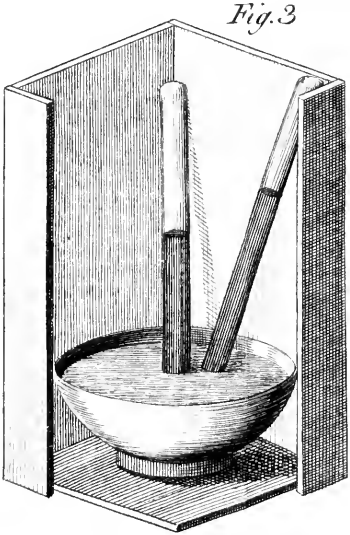
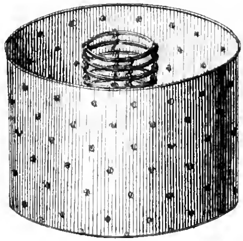


Fig. 4



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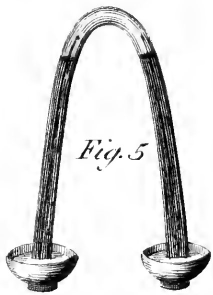

EXPERIMENTS

AND

OBSERVATIONS

RELATING TO VARIOUS BRANCHES OF

NATURAL PHILOSOPHY;

WITH

A CONTINUATION

OF

THE OBSERVATIONS ON AIR.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

In nova fert animus mutatas dicere formas
Corpora. ——— OVID.

LONDON:
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M DCC LXXIX.

TO

SIR GEORGE SAVILE, Bart.

THIS WORK

IS,

WITH THE GREATEST RESPECT,

INSCRIBED,

BY HIS MOST OBLIGED,

HUMBLE SERVANT,

J. PRIESTLEY.

THE

PREFACE.

AFTER the intimation given in the preface to my third volume of *Obſervations on different kinds of Air*, publiſhed about two years ago, viz. "that "I ſhould certainly give myſelf and my "readers ſome reſpite; foreſeeing that my "attention would be ſufficiently engaged "by ſpeculations of a very different na"ture;" ſome apology may be expected for obtruding on the public another volume of experiments, and eſpecially ſo large a one as that which is now before them.

In this caſe, however, it may be ſufficient to alledge the inſtability of human purpoſes and purſuits. But the ſpeculations referred to, which were of a metaphyſical nature, did not happen to engage ſo much of my attention as I expected, and did not, at any time, much interfere with my philoſophical purſuits. It is alſo to be obſerved, that that kind of writing is a thing of a very different nature from this. I can truly ſay (nor will it be thought extraordinary by thoſe who conſider the nature of theſe ſubjects) that ſingle ſections in this work have coſt me more than whole volumes of the other; ſo great is the difference between writing from the head only, and writing, as it may be called, from the hands. To the former little or nothing is requiſite but calm reflection; whereas to the latter much *labour*, and *patience*, and conſequently much *time*, as well as *expence*, are neceſſary.

I have, beſides, been engaged farther than I expected in philoſophical ſtudies by the proſecution of ſome inquiries which I had left unfiniſhed before, and eſpecially by the repetition of proceſſes the reſults of which had been queſtioned by others. Various other circumſtances, of which mention is made in the courſe of the work, likewiſe contributed

contributed to lead me ſtill farther in the ſame path. And having acquired a fondneſs for experiments, even ſlighter inducements than I have had would have been ſufficient to determine my conduct.

But in this I would by no means be underſtood to be making an *apology* for what I have done. I am far from conſidering the buſineſs of philoſophy as a thing that is cenſureable, or requiring any apology. On the contrary, though I do not conſider theſe ſtudies as the firſt in rank and value, I think their importance is generally much under-rated; and I earneſtly wiſh that more attention was given to them by thoſe who have ability, leiſure, and the neceſſary means for proſecuting them. For it muſt be acknowledged that in theſe ſtudies mere *genius* can do nothing without the aid of wealth. Indeed, *ſpeculation*, without *experiment*, has always been the bane of true philoſophy.

I am ſorry to have occaſion to obſerve, that natural ſcience is very little, if at all, the object of education in this country, in which many individuals have diftinguifhed themfelves fo much by their application to it. And yet fcientifical purfuits have fuch an advantage over moft others, as ought more efpecially to recommend them to perfons of rank and fortune. They never fail to furnifh materials for the moft agreeable and active purfuits, and fuch as are, at the fame time, in the higheft degree, ufeful and honourable, and are, by this means, capable of doing unfpeakably more for them than the largeft fortunes can do without this refource. Were perfons thus engaged, there would be lefs temptation to have recourfe to pleafure and diffipation, for the employment of their vacant time; and fuch purfuits would be particularly valuable to thofe who have no *talent* for politics, or any proper *call*, to occupy themfelves in public affairs. Befides, the laft is a path in which, from the nature of things, only a very few can walk; and the former, *viz.* a courfe of vicious pleafure, it is much to be lamented that any human being fhould tread.

Man

Man is a being endued by his creator with excellent faculties, and not to have *ſerious objects of purſuit* is to debaſe and degrade himſelf. It is to rank himſelf with beings of a lower order, aiming at nothing that is much higher than the low pleaſures they are capable of; at the ſame time that, from the remains of nobler powers, of which he cannot wholly diveſt himſelf, he is incapable of that unallayed enjoyment of ſenſual pleaſures that brutes have.

I ſhall not repeat, in this place, what I have advanced in favour of ſcientifical purſuits, as peculiarly proper for perſons of large fortunes, in the preface to my *Hiſtory of Electricity*, and my late *Obſervations on Education*; but would obſerve that, if we wiſh to lay a good foundation for a philoſophical taſte, and philoſophical purſuits, perſons ſhould be accuſtomed to the ſight of experiments, and proceſſes, in early life. They ſhould, more eſpecially, be early initiated in the theory and practice of *inveſtigation*; by which many of the old diſcoveries may be be made to be really *their own*; on which account they will be much more valued by them. And, in a great variety of articles, very young perſons may be made ſo far acquainted with every thing neceſſary to be previouſly known, as to engage (which they will do with peculiar alacrity) in purſuits truly original.

At all events, however, the curioſity and ſurprize of young perſons ſhould be excited as ſoon as poſſible; nor ſhould it be much regarded whether they properly underſtand what they ſee, or not. It is enough, at the firſt, if ſtriking facts make an impreſſion on the mind, and be remembered. We are, at all ages, but too much in haſte to *underſtand*, as we think, the appearances that preſent themſelves to us. If we could content ourſelves with the bare knowledge of new *facts*, and ſuſpend our judgment with reſpect to their *cauſes*, till, by their analogy, we were led to the diſcovery of more facts, of a ſimilar nature, we ſhould be in a much ſurer furer way to the attainment of real knowledge.

I do not pretend to be perfectly innocent in this refpect myfelf; but I think I have as little to reproach myfelf with on this head as moft of my brethren; and whenever I have drawn general conclufions too foon, I have been very ready to abandon them, as all my publications, and this volume in particular, will evidence. I have alfo repeatedly cautioned my readers, and I cannot too much inculcate the caution, that they are to confider new *facts* only as difcoveries, and mere *deductions* from thofe facts, as of no kind of authority; but to draw all conclufions, and form all hypothefes, for themfelves.

Having now begun a new work, it may perhaps be expected, by thofe who are pleafed to think favourably of my paft labours, that I fhould proceed with the fame fuccefs. But nothing can be more uncertain than this. I before compared philofophizing to hunting; and though hitherto I have been

been pretty fortunate, I may hereafter follow the chaſe to very little purpoſe. All I can ſay is, that I ſhall think myſelf happy to have *leiſure*, and the *means* of proſecuting theſe inquiries; and that I ſhall certainly, by ſome chanel or other, account to the public, in proper time, for whatever ſucceſs I may meet with.

I ſhall conclude this preface with obſerving, that the Abbé Fontana having heard that I had found pure air in water, was ſo obliging as to ſend me an account of ſome experiments of his, made at Paris, above a year ago, in confirmation of the ſame thing. He extracted by heat pretty pure air from ſeveral kinds of water, but eſpecially diſtilled water; though far ſhort of the purity of that which I procured in the circumſtances mentioned ſect. xxxiii. One meaſure of the beſt that he procured, mixed with two meaſures of nitrous air, occupied the ſpace of 2.5 meaſures; whereas the ſame meaſures with mine, as will be ſeen,

ſeen, occupied the ſpace of little more than half a meaſure. He alſo does not mention his having obſerved the difference in the quality of air extracted from water in conſequence of expoſure to the air, or the ſun.

London, March 1, 1779.

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THE

THE

INTRODUCTION.

HAVING, in the *Introductions* to my three volumes on the ſubject of *the different kinds of air*, noticed the improvements I had made in my apparatus, with the new proceſſes I had made uſe of, and explained the figures proper for that purpoſe, I ſhall do the ſame in this treatiſe. I have not, indeed, any thing of much importance to deſcribe; but to perſons who have many experiments to make, and who have little time to give to them, ſmall improvements are often of no ſmall value.

Fig. 1. repreſents the *ſhelf* on which I place the jars in my trough of water, and which is formed on the plan of that of the Duc de Chaulnes, with a ſmall addition. It is made to be fixed higher or lower in the

the water, as occaſion may require, by means of three bent pieces of copper or iron, on which it is ſuſpended; having ſmall wedges, or pieces of wood of different ſizes, for them to reſt on. The ſhelf is about an inch and an half in thickneſs, for the convenience of excavating the under-ſide in the form of *funnels*, the orifices of which, about a quarter of an inch in diameter, appear on the upper ſide, as the form and ſize of the cavity below is expreſſed by the dots above.

The funnels ſhould be made as capacious as poſſible; but care ſhould more eſpecially be taken, that no part of them be too flat, leſt any bubbles of air ſhould be retained, and not paſs into the veſſels placed to receive them.

When freſh air is generated, it is convenient to introduce the tube of the phial in which it is produced, quite under the ſhelf, into the hollow of the funnel. But when it happens that the ſweep of the tube is too ſhort for that purpoſe, I make uſe of a ſmall production of the upper part of the

ſhelf,

ſhelf, with a ſlit in it, under which the ſhorter tube may be brought; and the edge of the jar that receives the air, may be made to ſlide over the place at which the bubbles iſſue.

Fig. 2. is a ſide view of a glaſs *funnel* ſupported by a wooden pillar, riſing from a baſe, to which a plate of lead is faſtened, in order to make it ſink, and keep its place in the water. At the top of the pillar is a piece of wood cut in front (but, for that reaſon, not viſible in this figure) in a concave form, for ſupporting a glaſs tube, that, reſting on the orifice of the funnel, may lean againſt it. Both this piece of wood, and alſo that which ſupports the funnel, are made to ſlide up and down, and are fixed by wedges at whatever height is found to be moſt convenient. This apparatus ſaves the trouble and inconvenience of keeping one's hand in the water for the ſake of holding the funnel, while the air is pouring through it.

Fig. 3. repreſents an apparatus that would not deſerve a copper-plate, but that there

is

is often great convenience in little things. It exhibits a bafon of quickfilver, fo placed, in a frame of wood, as to contain feveral glafs tubes, which may be fupported with little trouble, and difpofed of without materially interfering with each other. In this manner I have often more than half a dozen in ufe at the fame time.

Fig. 4. reprefents a *cylindrical veffel* made of tin, inclofing another of iron wire. In the outer veffel a charcoal fire may be made, furrounding the inner cylinder, which, being open at the bottom, will admit the upper part of a glafs jar, fupported in whatever manner the operator may find moft convenient. Thus a jar, with the air, &c. contained in it, may be heated as much as the glafs will bear, without giving more heat than is neceffary to the lower part of it. In this manner alfo, an equal degree of heat may be given to every fide of the upper part of the glafs.

Fig. 5. explains the manner in which I make an electrical explofion pafs through any fubftance in the form of vapour. It reprefents

repreſents a glaſs ſyphon, in each leg of which is an iron wire, of ſuch a length, that there ſhall only be about half an inch between the heads of them. The ſyphon muſt be filled with mercury, and each of the legs inſerted in ſeparate baſons, alſo containing mercury. After this, the ſubſtance may be introduced into the ſyphon by means of a glaſs tube, and, being lighter, it will take its place in the bend of the ſyphon; which may then be placed near the opening of a ſmall furnace, or in the apparatus deſcribed Fig. 3. when whatever lodges in the upper part of the ſyphon will be converted into vapour, and the exploſion will be made in it by making the ſyphon part of an electrical circuit. Mercury itſelf may be converted into vapour in the ſame manner.

There is a great variety of methods of mixing nitrous and common air, in order to aſcertain the purity of the latter; among which that contrived by Mr. Magellan has the recommendation of much ingenuity, as well as much ſimplicity. But the manner in which I have been accuſtomed to perform that operation is ſtill more ſimple, though it has nothing to boaſt of with reſpect to ingenuity. It is neceſſary to deſcribe it, becauſe it is referred to through the whole of this work.

I firſt provide a phial, containing about an ounce of water, which I call *the air meaſure.* This I fill with air by having firſt filled it with water, and placed it over the opening of the funnel in my ſhelf; and when it is filled I ſlide it along the ſhelf, always obſerving that there be a little more air than I want. The phial being thus exactly filled with the air which I am about to examine, and care being taken that it be not warmed by holding in the hand, &c. I empty it into a jar about an inch and an half in diameter, and then introduce to it the ſame meaſure of nitrous air, and let them continue together about two minutes. I chuſe to have an overplus of nitrous air, that I may be ſure to have phlogiſton enough to ſaturate all the common air. If I find the diminution with theſe meaſures

 to

to be very confiderable, I introduce another meafure of nitrous air; but the pureft dephlogifticated air will not, I believe, require more than two equal meafures of nitrous air.

Sometimes I leave the common and nitrous air in the jar all night, or a whole day; but always take care that, whatever kinds of air I be comparing together, they remain the fame fpace of time before I proceed to note the degree of diminution.

When the preceding part of the procefs is over, I transfer the air into a glafs tube, about three feet long, and one third of an inch wide, carefully graduated according to the air-meafure, and divided into *tenths* and *hundredth parts*; fo that one of the latter will be about a fixth or an eighth of an inch. Then immerfing the tube in a trough of water, fo that the water in the infide of the tube fhall be on a level with the water on the outfide, I obferve the fpace occupied by them both, and exprefs the refult in *meafures* and *decimal parts of*

a meafure,

a meaſure, according to the graduation of the tube.

It is ſome trouble to graduate a tube in this manner; but when it is once done, the application of it is extremely eaſy. As it will ſeldom happen that a glaſs tube is of an equal diameter throughout, I generally fill that part of the tube which contains one meaſure, with quickſilver, and then weighing it, and dividing it into ten parts, put them in ſeparately, in order to mark the primary diviſions. This operation is performed very readily by having a glaſs tube drawn out to a fine orifice, in order to take up a ſmall quantity of quickſilver at a time, as it may be wanted.

OBSERVATIONS

RELATING TO

VARIOUS BRANCHES

OF

NATURAL PHILOSOPHY.

SECTION I.

Obſervations relating to the NITROUS ACID, *and eſpecially the* COLOUR OF IT.

IN my third volume of *Obſervations on Air*, I related ſeveral experiments to aſcertain the ſtrength of the nitrous acid, as depending upon the circumſtances in which it was made, and others relating to the *colour* of it, eſpecially when I made this acid by impregnating diſtilled water with the nitrous vapour; in which caſe it firſt became blue, then green, and laſtly yellow. I alſo obſerved that, at the be-

ginning

ginning of the common proceſs for making nitrous acid, it was frequently a little orange coloured, then a pale yellow, and at the laſt orange coloured again; but that a little phlogiſtic matter in the materials would always make the whole produce of a deep orange colour. I have ſince that made many more obſervations relating to the colour of this acid; and I think I have deciſively proved, that neither this acid, nor the muriatic, have, naturally, any more colour than the vitriolic acid, or than water itſelf; being able to give them colour, change it, or wholly take it away at pleaſure; and ſome of the circumſtances in which theſe changes take place are not a little remarkable.

The facts that I ſhall relate prove that it is either *phlogiſton*, or *mere heat*, that gives colour to this acid, that this colour may alſo be all expelled by heat; but that continuance of heat will give it more colour, and deepen it at pleaſure, ſo that more heat, in glaſs veſſels hermetically ſealed, ſeems to have the ſame effect with phlogiſton. But, more

more probably, heat affects it in ſuch a manner, as to develope, as it were, the phlogiſton it contained before, and put it into *a new ſtate,* rendering that part of the acid to which it is attached both more volatile, and alſo diſpoſed to reflect the rays of light in a particular manner; whereas, before this action of the heat, the phlogiſton was *latent*, at leaſt, did not evidence itſelf by thoſe particular effects.

On the firſt of Auguſt 1777, I reſumed my experiments on this ſubject; when, having provided a ſand furnace, to be kept hot for a conſiderable time, for many purpoſes that will be mentioned in the courſe of this volume, I put a quantity of ſtrong and pale coloured ſpirit of nitre into a glaſs tube, about an inch in diameter, and three feet long; and, ſealing it hermetically, I placed it in the warm ſand. Taking it out after ſome time, I found it orange coloured; and though it was more deeply coloured while it continued hot than it was afterwards, it retained ſo much of the colour, as to be ever after of as deep an orange colour as ſpirit of nitre is generally found to be. And though before this proceſs the vapour riſing from it was quite colourleſs, there being nothing viſible above the ſurface of the acid, in the phial from which it was taken, the whole tube (which I have obſerved was three feet in length) was uniformly filled with the dark orange coloured vapour.

This proceſs being performed in a glaſs tube hermetically ſealed, I was fully ſatisfied, that this colour which the acid had aſſumed could not be owing to any thing beſides heat. That it was not owing to any thing peculiar to the glaſs of lead, of which, in a great meaſure, flint glaſs conſiſts, was evident from obſerving the ſame effect on the acid when the experiment was made in common green, or bottle glaſs.

Having about the ſame time expoſed to a heat of ſome continuance ſeveral quantities of blue and green ſpirit of nitre, it may not be improper to note the reſults of theſe experiments in this place. In one inſtance, the green ſpirit of nitre became orange coloured; but when it was cold it was almoſt as green as at firſt, though there was evidently a mixture of yellow in it.

When I had expoſed a quantity of blue ſpirit of nitre in a long glaſs tube a few days, the blue colour was barely perceivable. It was placed in the ſand furnace on the 23d of Auguſt, and on the 30th of September following it was entirely colourleſs, and had no viſible red vapour over it when cold. This acid was very weak, otherwiſe, I doubt not, the yellow or orange colour would have become apparent.

I alſo expoſed to a very moderate heat a ſmall phial with a ground ſtopper, almoſt filled with a deep blue nitrous acid, when it preſently aſſumed a deep green, and when it was cold it reſumed its former blue colour. In this experiment the heat had not been continued ſufficiently long to produce a permanent change of colour. For, having expoſed to a moderate heat, in a long glaſs tube, hermetically ſealed, a quantity of blue nitrous acid, it loſt its blue colour, and aſſumed a yellow one;

and when it was cold the blue colour did not return, except in the ſmalleſt degree.

I did not, however, come to this concluſion concerning the cauſe of the change of colour in this acid in the ſummary manner above deſcribed; but in conſequence of a ſeries of obſervations, attended with a variety of circumſtances, ſome of which were remarkable enough.

A little time before I had made the experiments above recited, I had begun a new mode of examining a variety of fluid ſubſtances; which was to put a ſmall quantity of the fluid into a glaſs tube, three or four feet long, and ſealing it hermetically, to expoſe the end containing the fluid to as great a degree of heat as I found it could bear; and to keep it in that heat a conſiderable time. My deſign in providing tubes of this length was to give room enough for the vapour to expand, and condenſe in the remote and cool end of the tube, while it was boiling in the other end.

In this manner I expoſed to the influence of heat a ſmall quantity of ſpirit of nitre, as I had done a variety of other fluid ſubftances, without any particular expectation.

The acid, however, no fooner felt the heat than it exhibited appearances that engaged my attention very ftrongly; and whoever will repeat the experiment in the fame manner in which I firft made it, will find it a very pleafing one.

The fpirit of nitre I made ufe of was of the ftrongeft and paleft fort, without the leaft perceivable red vapour over the furface of it. The glafs tube in which it was confined was about four feet long, and about one third of an inch in diameter, and the fpace occupied by the acid was two inches in length. The tube thus prepared I held in my hand, prefenting the end in which was the fpirit of nitre to a common fire, and holding the tube in an inclined pofition. The firft effect of the heat to which it was expofed, was its affuming an orange colour throughout. After this, a red, or deep orange coloured vapour, appeared above the furface of the acid, and gradually afcended higher into the tube, at the fame time that the acid itfelf grew paler, and at length became quite colourleſs, like water, all the colouring matter being, to appearance, driven out of it.

This red vapour kept riſing higher and higher in the tube, leaving a conſiderable ſpace, ſome times of ten or twelve inches, between it and the acid, all which ſpace was quite tranſparent. This was a very pleaſing appearance, and it was amuſing to obſerve the ſpace occupied by the red vapour, which extended three or four inches, every thing elſe in the tube above and below it being tranſparent, and the red ſpot itſelf receding from the acid as the heat increaſed, or approaching to it as the heat diminiſhed.

I obſerved, however, that by the continued application of heat the quantity of red vapour increaſed, and the colour grew manifeſtly deeper; when, beginning to apprehend (though, as I found afterwards, without any reaſon) that the tube might burſt, I withdrew it from the fire, and preſently ſaw the red vapour deſcend lower and lower, till it reached the colourleſs

acid

acid at the bottom of the tube, and, entering into it, communicated to it its own orange colour. But when it was quite cold, did not, at that time, perceive that the acid was of a deeper colour than it had been at the commencement of the proceſs, and no viſible vapour remained upon it. To produce a permanent colour, as I obſerved before, more *time* was requiſite.

When one of theſe tubes had been thoroughly heated two or three times, and the laſt time had been expoſed to a boiling heat for about an hour (the heat having been ſuch as to keep the acid quite colourleſs, and likewiſe to make a large colourleſs ſpace above the acid) I let it cool in a very good light, and then obſerved, that as the red vapours deſcended, and the condenſed liquor, highly charged with it, trickled down the tube, and mixed with the colourleſs acid below, it made *waves* in the acid, ſomething like oil in water, or rather like the mixture of a ſtrong acid in water, and that this denſer acid deſcended in theſe viſible waves to the very bottom of the liquor;

liquor; and yet when the depth of the acid was about two inches, the upper part was ſenſibly darker coloured by this means than the lower. I alſo obſerved, that while the acid was acquiring its colour, as long as it continued tolerably warm, a vapour kept iſſuing out of it, and dancing in a beautiful manner to the height of an inch, or two inches, above the ſurface of it.

In order to obſerve the difference that might be occaſioned by expoſing the acid to a greater and longer continued heat, I kept one of theſe tubes ſo as to boil violently, and be quite colourleſs, for a conſiderable time, while I kept another of them in ſo moderate a degree of heat, as only to make the acid of a deep orange colour, but never to expel the red vapour from it. After ſome time that which had boiled violently remained of a deeper orange colour than the other, and the tube continued to be filled with the red vapour after the experiment. In both theſe tubes the acid retained a manifeſtly orange colour when it was quite cold, and kept it ever after.

after. The tube that had been expoſed to the greateſt degree of heat continued alſo quite full of red vapour, and the quantity of the liquor was diminiſhed about one twentieth part, the reſt being probably combined with the red vapour, or diſperſed in the tube, ſo as not to be collected again.

I have at the time of this writing ſeveral tubes in which this proceſs has been performed, one of which is an inch wide, and three feet long; and though it had only a ſmall quantity of acid in it, originally of a pale colour, and without any viſible vapour, the whole of that large tube is filled with the denſeſt orange coloured vapour expelled in this manner from the pale acid, and it has continued ſo more than a year, without any appearance of the vapour entering into the acid again; except that the colour of the acid, from being of a deep orange, which it retained a conſiderable time, is now become quite green. This is alſo the caſe with a pretty large quantity of the acid, which had been quite pale, but was made of a deep orange, by expoſure expofure to heat in glafs veffels hermetically fealed, and in that ftate transferred into a phial with a ground ftopper, and which has been kept clofe fhut near a year.

I had now tubes filled with the red vapour of fpirit of nitre exactly refembling thofe of which an account is given in my third volume of *Obfervations on Air*, which were made by the rapid folution of bifmuth in fpirit of nitre; and I found that thefe had the very fame property. For whatever part of thefe tubes I heated with the flame of a candle, it became of an intenfely orange, or red colour, while the parts both above and below it, which were not heated, remained unchanged.

Having been much pleafed with this expulfion of all the colouring matter from a quantity of fpirit of nitre; and feeing it in the form of vapour confined to the fpace of four or five inches, in the middle of a very long glafs tube, which was quite tranfparent above and below it, I made feveral attempts to feparate this coloured vapour from the fluid, out of which it had been expelled, by

melting

melting the tube in the intermediate colourlefs fpace, and fealing it hermetically. But thefe attempts were in vain, on account of the increafed expanfive force of the vapour in that heated ftate.

I imagined, however, that I might, by this means, when the tube was quite filled with red vapour, and cold, take it off from the acid, and preferve it red and dry, or nearly fo. But in attempting this, I prefently found that there had been a great increafe of elaftic matter within the tube. For the moment I had a little foftened a part of the tube, in order to take it off from the reft, the red vapour rufhed out with great violence; the effect of the heat on the phlogifton in the acid being fuch, as to render the vapour to which it was attached permanently elaftic, and incapable of being any more abforbed by the acid, from which it had been expelled. It is poffible, however, by this means, to get a tube filled with a moderately red vapour. But foon after I hit upon a much eafier method of effecting the fame thing.

Though

Though I could not ſeparate the red vapour from the colourleſs acid while it was boiling, it was very eaſy, I found, by boiling the acid in a ſhort tube, or phial, to expel all the colouring matter from it, and thus to get a quantity of ſpirit of nitre quite free from all colour; which I accordingly did, and then imagined that, the coloured vapour being wholly expelled from it, the acid would always continue colourleſs. And ſo, indeed, it did after it was quite cold; and it will continue without return of colour, and be but little diminiſhed in quantity, or impaired in ſtrength, ſo long as it is kept from the contact of any thing that contains phlogiſton, or from much heat. But, to my great ſurpriſe, at that time, I found that either of thoſe circumſtances would make this colourleſs acid reſume its former colour, or acquire a deeper one than it had before. It was, however, by accident, that I firſt learned this.

Having procured a quantity of nitrous acid quite colourleſs, I put a part of it into a phial which had a common cork (a phial with

with a glaſs ſtopper happening not to be at hand) and not ſuſpecting that this circumſtance would affect the colour of the acid, which was a conſiderable diſtance from the cork. I found, however, after two days, when I took out the cork, that the acid ſmoked very much, and had completely recovered its original yellow colour, ſo as not to be diſtinguiſhed at ſight from what it had been before the colouring matter had been expelled from it. I then took a part of this acid, and incloſing it in a glaſs tube, which I ſealed hermetically, expoſed it to the heat as before, when it became of an orange colour; and reſuming the proceſs in an open tube, I drove out the colouring vapour once more, and made the acid a ſecond time tranſparent.

I found, however, that a little phlogiſtic matter has a quicker and more remarkable effect on this colourleſs acid than mere heat. I put a part of the colourleſs acid into one of the tubes above mentioned, and kept it boiling a whole day before the fire, and the night following in a ſand heat,

heat, without being able to perceive any ſenſible change in it, though a ſlight redneſs was apparent on the firſt application of the heat. But having put another part of the ſame original quantity of the colourleſs acid (which from the preceding experiment will be judged to have been very weak) into a phial with a common cork, at the diſtance of an inch from it, I obſerved, that in a few hours only, the upper part of the acid was become yellow, and the next morning it was yellow throughout, exactly like the beſt nitrous acid when freſh made.

But no inſtance of a change of colour in this acid by heat was ſo very remarkable as the following. Having put a ſmall quantity of pale colourleſs acid, into a ſhort glaſs tube, and almoſt burying it in the hot ſand, I found the next morning, that the whole tube was quite filled with red vapour, and the acid itſelf was quite red, and perfectly opaque, and to appearance a little *viſcid*, like red ink. Neither before, nor ſince, have I ever ſeen nitrous acid in that ſtate. It even retained the ſame appearance which

which was not orange, but a proper and a very deep *red*. Being quite cold, I could examine it at my leiſure. It was the only appearance I ever had of the kind.

Replacing the ſame tube in the ſand heat, and taking it out ſome time after, the acid was of a deep orange while hot, but not very deep, and rather of a pale colour when cold; but there was a little whitiſh matter formed on different parts of the glaſs, of which a farther account will be given preſently.

I ſoon found that the *cloſe confinement* of the vapour contributed greatly to this change in the acid. A quantity of colourleſs acid being put into a ſhort thick tube hermetically ſealed, and placed in the ſand heat, in about an hour had red fumes, and in an hour more the acid was orange coloured. Whereas a quantity of the ſame acid confined in a *long tube* the ſame time, and in the ſame degree of heat, had acquired red fumes only, while the acid itſelf remained colourleſs.

In all the circumſtances in which much heat is given to ſpirit of nitre, it neceſſarily acquires a deeper colour. This is the reaſon why, in all my attempts to procure a very ſtrong ſpirit of nitre, by uſing concentrated vitriolic acid, and boiling the nitre, in order to expel the water it contained, it was always of an orange colour. For, in this caſe, the mixture of the oil of vitriol and nitre was attended with great heat.

I believe that any degree of heat, ſufficient to throw the acid into the form of vapour, will always give it more colour than it had before. This I found to be the caſe when I rediſtilled a quantity of ſpirit of nitre from freſh nitre, in order to purify it from any vitriolic acid that might remain in it. The reſult of this proceſs was an acid of a deeper colour, and that ſmoked more than it did before. It is poſſible, however, that a ſmall quantity of ſome matter containing phlogiſton might have been concealed in the nitre I made uſe of, though I had no particular reaſon to ſuſpect it.

Having procured nitrous acid in the ſeveral ſtates above-mentioned, *viz.* the original pale coloured acid, that out of which the

the colour had been expelled by heat, that which had been diſtilled again from freſh nitre, and that which had been phlogiſticated by heat in cloſe veſſels, I tried the ſtrength of them all by the ſolution of copper, meaſuring the quantity of nitrous air that equal bulks of them (all other circumſtances being the ſame) produced, and obſerved that a quantity of each occupying the ſpace of 2 dwts 18 grains of water yielded as follows, *viz.*

	Ounce Meaſures.
The original pale coloured acid, -	14
The colourleſs, -	11
That rediſtilled from nitre,	11
That coloured by heat,	11

This highly phlogiſticated acid hiſſed very much when mixed with water. The produce of air was more or leſs accelerated during the courſe of the ſolution in all of them, but moſt of all when I uſed the pale coloured acid. I muſt obſerve that, in making this colourleſs acid, I uſed more heat than was neceſſary, and therefore weakened it too much, though it is certainly

 impoſſible

impoſſible to expel the colouring phlogiſton without expelling, at the ſame time, the acid to which it is attached. It is ſome thing remarkable, that the phlogiſton, *in this particular ſtate*, ſhould attach itſelf wholly to one part of the acid only, though mixed with the reſt of the acid, combined alſo with phlogiſton, but in a different ſtate. Theſe experiments, however, ſufficiently demonſtrate this to be the caſe.

Heat is not neceſſary to make ſpirit of nitre colourleſs. For expoſure to the open air does the ſame thing, and probably with leſs diſſipation of the acid. During this expoſure to the open air, the nitrous acid, if it be ſtrong, increaſes conſiderably in bulk and weight, in which it reſembles the vitriolic acid, though this is not in the ſmalleſt degree volatile. In order to obſerve more diſtinctly the whole of this proceſs, ſome time in the month of July 1777, I expoſed to the open air, in a common glaſs tumbler, about three ounces of orange coloured ſmoking ſpirit of nitre. In a day or two it was quite

quite colourlefs, but a fly, or any fmall fubftance containing phlogifton, falling into it, would colour the furface of it again for a confiderable time, though at length thefe accidents had lefs effect upon it. This acid kept increafing in bulk to the April following, when the quantity was confiderably more than doubled; but from that time it began to decreafe, and continued fo to do till more than half that it had gained was gone, after which it continued very much the fame for feveral months.

The circumftances relating to the *white matter*, which I have obferved was formed by the nitrous acid in glafs tubes hermetically fealed, and expofed to a continued heat, I am not able to explain. I firft obferved it in that fhort tube in which the phenomena of the colour of the acid were fo very remarkable, and indeed fingular; but afterwards it never failed to make its appearance whenever the acid had been long confined, and expofed to much heat, but the quantity procured was too inconfiderable to make many experiments upon it.

It was on the 25th of September that I obſerved this white, or yellowiſh, matter in the tube above-mentioned. On the 30th of the ſame month, I obſerved that the colour of the acid was rather lighter, and beſide that whitiſh matter at the bottom of the tube, there was a ſimilar concretion adhering to the ſides of the glaſs, juſt above the ſurface of the acid, the colour of which was partly yellow, and partly green.

Having got more of this white matter in other tubes, I obſerved that it was eaſily ſcraped off from the glaſs, and left it tranſparent, ſo that it ſeems to be ſomething depoſited from the acid, and not an abraſion of the glaſs. It was not at all affected by diſtilled water, but ſpirit of ſalt diſſolved it entirely, and became of a yellow colour inclining to orange. Applying the flame of a candle to that part of the glaſs tube on which ſome of this white matter lay, it was diſſolved, and diſperſed in *white*, not *red* vapours. An earthy pellicle remained, not eaſily affected by heat, but it was diſperſed when it was made red hot with a blow pipe.

This

This pellicle adhered firmly to the glafs, but in time it was completely diffolved by fpirit of falt, which affumed the colour above-mentioned.

It is pretty evident, from this obfervation, that this matter did not really contain fpirit of nitre *as fuch*. For had it contained the proper nitrous acid combined with any earthy matter, as the calx of the lead in the glafs, the fpirit of falt could not, I apprehend, have decompofed it. In other refpects it had very much the appearance of minium become white by imbibing nitrous vapour. But this is not at all affected by fpirit of falt.

It was evident, however, that wherever this white matter was formed, the quantity of the acid was diminifhed, fo that it looks as if the acid itfelf was deftroyed, and converted into fomething of a different nature.

On the 6th of January 1778, I obferved that a long glafs tube, one fourth of an inch in diameter, into which I had put as much fpirit of nitre as filled about half an inch of it, and which had been expofed to the fand heat about two months, had no moiſture in it, except a very little that adhered to the ſides, too ſmall to run down the tube. The tube continued full of red vapour, and ſo it continued ſeveral months, but not ſo deeply coloured as it had been ſome time before, and about half an inch at the bottom of the tube had a ſlight incruſtation of the white matter mentioned above. That the volatile matter was diminiſhed, was evident from my obſerving that when I melted a part of the tube with a blow pipe, the glaſs was preſſed ſtrongly inwards, whereas before the formation of this white matter, when I ſoftened any part of the tube in this manner, the expanded vapour would burſt it open, and ruſh out with great violence.

After eight or ten months, I obſerved this tube to have loſt the greateſt part of its colour, and in a few weeks more it was quite colourleſs. Examining it more narrowly, I obſerved an exceedingly minute crack, about half an inch above the bottom of the tube. However, when I ſoftened

the

the glaſs with a blow pipe it was ſtrongly preſſed inwards, ſo that there ſeemed to have been little or no communication between the air within and that without. When that crack was made I cannot tell; and I muſt leave it to the opinion of my reader, whether it be probable, all circumſtances conſidered, that the acid had, in any meaſure, eſcaped by that crack.

I have obſerved, in my former publications, that common air is phlogiſticated by continuing a conſiderable time involved in the red vapour of ſpirit of nitre. This, contrary to my expectation, I alſo found to be the caſe with the colourleſs, or inviſible vapour of ſpirit of nitre, after all the colouring phlogiſtic matter had been driven out of it. Air that had continued only two days in a phial with a glaſs ſtopper, which contained ſome of this colourleſs acid, was ſenſibly leſs affected by nitrous air than common air was; and the air that had been confined in the ſame glaſs tube in which ſome of the colourleſs nitrous acid had been placed in the ſand furnace only two days, though

though the heat had been ſo ſmall as to have produced no change of colour in the acid, was ſo much phlogiſticated, that one meaſure of it, and one of nitrous air occupied the ſpace of 1.81 meaſures.

SECTION II.

Of the Nitrous Acid Vapour.

IN the third volume of *Obſervations on Air*, I obſerved the remarkable effects of impregnating oil of vitriol with nitrous acid vapour. It was one of the laſt obſervations that I made before the printing of that volume.

Having impregnated a larger quantity of the oil of vitriol than I made uſe of in thoſe experiments, I left ſome of it in a large phial, with a ground ſtopper, among other phials containing things for which I had no immediate uſe. But though *my* proceſs was over, that of *nature* was not. Happening to be looking at it on the 19th of March following, perhaps about ſix months

months after the impregnation, I found what I was far from having expected, *viz.* that almoſt the whole was cryſtallized, a very ſmall part only of the contents of the phial remaining liquid. The cryſtals looked exactly like ice, and exhibited all the appearances that I had before obſerved to attend the ſimple impregnation of the vitriolic acid with nitrous vapour, but in a much more elegant manner. For on dropping a piece of this ice into pure water, it became green, and efferveſced with great violence; and, what made a beautiful and ſtriking phenomenon, all the water in which the ice was diſſolved began inſtantly to ſparkle, with the ſpontaneous and copious production of air. With the help of a little heat, this production of air was ſo great, that the quantity was more than a hundred times the bulk of the ice that had been diſſolved. It was the pureſt nitrous air. In fact, a great quantity of nitrous vapour was, as it were, impriſoned in this oil of vitriol, and being ſuddenly ſet looſe, on being plunged in the water, it impregnated

nated the water in the ſame manner as I have obſerved that the nitrous vapour never fails to do.

The application of heat made this ice emit a denſe red fume; but holding a quantity of it in a glaſs veſſel over a candle, it preſently melted, emitting bubbles; and then, letting it ſtand to cool gradually, it cryſtallized very ſuddenly, when it was about blood warm. It was in this ſecond congelation much more opaque, and denſer than it had been in the former. When this ice was diſſolving with heat, the fume it emitted was not red, but white, and exceedingly denſe, like oil of vitriol in vapour. After it had been kept diſſolved, and in a boiling heat, ſome time, it did not cryſtallize afterwards, but continued fluid and tranſparent; being then, probably, mere oil of vitriol.

I have not yet been able to inveſtigate all the circumſtances neceſſary to this remarkable cryſtallization, having originally found it when I had no expectation of any ſuch thing, and having often failed to find it when

I have

I have expected it the moſt. All that I can do, therefore, is to recite what I have obſerved, with all the circumſtances that I can recollect relating to the appearances.

I had kept about half an ounce meaſure of oil of vitriol, not quite ſaturated with nitrous vapour, in a ſmall phial, with a ground ſtopper, about a year, in all which time it had ſhewed no tendency to cryſtallization, and from its imperfect impregnation I had not expected it. I was intending to complete the impregnation, and, looking at the phial, had taken out the ſtopper, and put it in again, deferring the proceſs till the day following, when I found the phial almoſt filled with the moſt beautiful cryſtallizations imaginable.

Their form, as nearly as I can deſcribe it, was that of a feather. They were about twenty in number, ſome of them as large as the phial could contain, and many of them parallel to each other, but others lying in different directions. The two parts, as it were, of the feather made an angle with each other of about 160 degrees, and each of of the ſingle fibres that compoſed the feather, but which were connected, like the toes of a duck's foot, by the ſame ſubſtance (but thinner, and more tranſparent than the reſt) made an angle with the ſtem from which they aroſe of about 45 degrees. A more beautiful appearance can hardly be imagined, and I am afraid I ſhall never ſee the like again.

Having obſerved theſe cryſtals ſome days, and ſeeing no farther change in them, or in the liquor which covered them, and which roſe about a quarter of an inch above them, I poured the liquor from the cryſtals, and for ſome time they continued upright, exhaling a red vapour, which filled the phial, and at length very much clouded and obſcured it. This liquor exactly reſembled ſtrong ſmoking ſpirit of nitre, and ſeemed to have nothing of the vitriolic acid in it.

After ſome time the cryſtals ſeemed to decay, and ſunk down in the phial, filling up all the interſtices that had been among them, ſo as to make one compact maſs, without

any

any thing of the beautiful appearance that they made before. Hoping to repair the injury they had ſuſtained, and to reſtore their beauty, I filled up the phial with freſh oil of vitriol ſtrongly impregnated with nitrous vapour, but it had no ſenſible effect, nor did any more cryſtals of the ſame, or of any other form ſhoot out from them in many months.

Having another phial of oil of vitriol partly impregnated with nitrous vapour, and of about the ſame ſtanding with the former, I examined it, and found it half filled with cryſtals, but theſe lay all confuſedly at the bottom of the phial, and though in ſeparate pieces, of no uniform ſhape.

After this I impregnated three different quantities of oil of vitriol with nitrous vapour. One was very ſtrongly concentrated, having diſtilled off about half the quantity of the beſt common ſort, the ſecond was both diſtilled and concentrated, and the third was only of a medium ſtrength, and the common ſort, but colourleſs. I kept all theſe in the ſame ſituation, and in about a fortnight that which had been ſimply concentrated began to cryſtallize, and in about a fortnight more the phial was half filled with cryſtals, ſome of them in the form of feathers, but lying in different directions, and not detached from each other; but forming a compact maſs.

In this ſtate I left them, being obliged to be abſent from my laboratory about three months; and at my return I found all the phials full of cryſtals, but generally in ſolid maſſes, with few ſuch feathers as I have deſcribed above, and theſe very ſhort ones.

Imagining that this ſingular cryſtallization might poſſibly be accelerated by expoſing the impregnated vitriolic acid to heat, I took a quantity of it which had continued a conſiderable time without cryſtallizing, and confined it in a glaſs tube three feet long, and half an inch in diameter. Then holding it to the fire, I obſerved that the acid emitted red vapour, which filled the whole tube, exactly as would have been the caſe with ſpirit of nitre

nitre itſelf. When it was cold many ſmall cryſtals were ſcattered all over the tube above the ſurface of the liquor, and the upper part of it was red; being, I ſuppoſe, the ſpirit of nitre that had been driven out of it by the heat, as being more volatile than the vitriolic acid.

I have already obſerved that, to appearance, the vitriolic acid impregnated with nitrous vapour was nothing but nitrous acid, after the complete formation of the cryſtals, and by experiment I found it to be nothing elſe. For diluting it with water, and diſſolving iron in it, in a phial with a ground ſtopper and tube, in the manner in which I uſually produce nitrous air, it yielded this kind of air only, without any mixture of inflammable air; which I have formerly obſerved is the caſe when the vitriolic and nitrous acids are mixed together, and employed in the ſolution of iron, the nitrous air coming firſt, and the inflammable air afterwards.

Here, indeed, a very ſmall quantity of the laſt produce burned with a lambent flame; but this I have obſerved to be the caſe with the laſt produce from iron and the nitrous acid only, when the proceſs was urged, as it was now, with the flame of a candle. The water, when this acid was mixed with it, ſparkled very much, yielding, I doubt not, nitrous air. But this circumſtance only proves it to have been highly charged with phlogiſticated nitrous vapour.

Here then is a caſe in which the nitrous acid appears to have a ſtronger affinity with water than the vitriolic: for in a courſe of time, it intirely expells the vitriolic acid from it, and unites with it itſelf; all the vitriolic acid being precipitated in the cryſtals that conſiſt of both the acids.

At the time of my laſt publication, I filled tubes and phials with the red nitrous vapour, by means of the rapid ſolution of biſmuth in ſpirit of nitre, which is a troubleſome operation, when the tube is to be ſealed hermetically after being filled with the vapour. The manner in which I ſucceeded in this experiment would be te-

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dious to defcribe, and it would be unneceffary, as I have fince effected the fame thing in a much eafier manner. For red lead converted into a white fubftance (as I have obferved it to be by impregnation with the nitrous vapour, and which may be kept in that ftate without deliquefcing any length of time, and without feeming to be difpofed to part with any of the vapour which it has imbibed in the temperature of the atmofphere) readily emits it in a melting heat. I therefore put a fmall quantity of this white minium into a glafs tube clofed at one end; then, holding it to the fire, make it emit the red vapour, till the whole tube is filled with it; and having the other end of the tube drawn out ready for clofing, as foon as the vapour begins to iffue out of that end, I apply my blow pipe and feal it.

By this means I conclude that the tube is filled with a pure red vapour, without that mixture of nitrous air, and perhaps common air alfo, which I could not exclude before; and when this is done, I can eaſily, afterwards, melt off that part of the tube which contains the minium, ſo that it does not at all appear in what manner the tube was filled with the vapour. A tube thus prepared will become of a deeper colour with heat, and paler with cold, exactly as the tubes filled in the manner deſcribed in my third volume. A little moiſture is expelled from the white minium along with the red vapour, but it is very inconſiderable.

This white minium never fails to be produced when, in any circumſtances, the common minium is ſufficiently impregnated with nitrous vapour. In making a quantity of dephlogiſticated air from the common minium and ſpirit of nitre, I once filled a whole gun barrel with the materials; and when I emptied it, after the proceſs, in which the bottom of the gun barrel only had been affected with the heat, I found part of the minium, at a ſmall diſtance from the place that had been the hotteſt, perfectly white, while that from which the air had been expelled was yellow,

yellow, as uſual, and that which was farther from the heat than the white minium was almoſt black.

Having had a ſlight ſuſpicion that the whiteneſs of this minium might poſſibly have been occaſioned by ſomething from the *biſmuth*, carried over along with the nitrous vapour produced in the ſolution of it, I made a ſimilar proceſs with the ſolution of *iron*, and found that it had the very ſame effect as the ſolution of biſmuth, converting the minium into a white ſubſtance, exactly like that which I had procured before. It is, therefore, the pure effect of impregnation with nitrous vapour, but certainly a very extraordinary one, and it may be well worth while to extend the proceſs to various other ſolid ſubſtances.

Since my laſt publication I have impregnated ſeveral other liquid ſubſtances with nitrous vapour, and the reſults in ſome of the caſes are not a little remarkable, eſpecially with reſpect to the colour communicated by this means.

The phoſphoric acid is preſently ſaturated with nitrous vapour, and aſſumes a deep indigo blue colour.

Radical vinegar is alſo ſoon ſaturated with this vapour, and aſſumes a light blue.

Spirit of ſalt ſaturated with freſh minium, ſo as to be of a yellow colour, becomes of a deep orange when impregnated with nitrous vapour.

Spirit of ſalt ſaturated with white minium, made ſo in conſequence of the colour being extracted from it by the ſpirit of ſalt, aſſumes a light blue colour by being impregnated with this vapour.

Spirit of ſalt ſaturated with red precipitate, or the precipitate *per ſe*, aſſumes a green colour.

Spirit of ſalt ſaturated with flowers of zinc acquires a blue colour, deeper than a ſky blue, but not ſo dark as the blue of the phoſphoric acid.

SECT.

SECTION III.

Some Phenomena attending the Solution of Metals in Nitrous Acid.

AS the difcovery of fixed air in calcarious fubftances threw new light upon many phenomena in chemiftry, in like manner the difcovery of every other kind of air, and indeed of every property of any of them, muft throw light upon thofe proceffes in which they are concerned. Not being a profeffed chemift, and attending only to fuch articles in that branch of knowledge as my own purfuits are particularly connected with (though thefe neceffarily grow more various and extenfive continually) fuch illuftrations of chemical proceffes are not fo likely to occur to me, as they are to others, who by their profeffion give a general attention to every thing within the whole compafs of chemiftry. Such, however, as I have had occafion to attend to, and which I imagine I can throw any light upon, I ſhall not fail to mention.

There are ſeveral facts relating to the ſolution of metals in ſpirit of nitre, which could not have been underſtood without the knowledge of nitrous air; and yet, though ſeveral of them are very remarkable, I do not find that even the phenomena themſelves, and much leſs the difficulties attending the ſolution of them, have been ſo much as noticed. I am perſuaded, however, that an attention to the nature of this remarkable kind of air will contribute greatly to the inveſtigation of the conſtitution of the ſeveral metals, and the explanation of many phenomena attending their decompoſition, and conſequently their compoſition.

Having had frequent occaſion to diſſolve mercury in ſtrong ſpirit of nitre, in order to procure from it nitrous and dephlogiſticated air, and to note the quantity of the metal revivified afterwards, I could not help being very particularly ſtruck with ſome phenomena

phenomena in the ſolution, which are as follows.

The moment that ſtrong ſpirit of nitre is poured upon quickſilver, the ſolution is inſtantly very rapid. But though it is known that one method of procuring nitrous air is by the ſolution of this metal in the nitrous acid, not a ſingle bubble of any kind of air is ſeen to be formed; at leaſt none riſes through the acid. Preſently, however, one may perceive, that very large bubbles of air *are* formed, but they inſtantly diſappear, and nothing remains of them but the ſmalleſt ſpecks imaginable, to riſe to the top of the acid. By degrees, the acid near the mercury becomes of a deep orange colour, and then through this part of the acid the bubbles of air aſcend freely; but the moment they come to the ſuperincumbent pale coloured acid, they collapſe into thoſe ſmall and barely perceivable points, yielding no air that can be collected in any ſenſible quantity. And it is not till the whole quantity of the acid is changed from a pale to an orange colour, that any nitrous air can be collected. Then, however, the bubbles rise freely to the top of the acid, and, mixing with the incumbent common air exhibit an orange colour by their decomposition on mixing with it. Then, also, a strong smell of spirit of nitre is perceived, as it always happens when nitrous air is let loose to mix with the air of the room in which we are breathing. Whereas, immediately before, no smell was perceived, and the common air incumbent on the mixture was quite colourless.

Had these singular phenomena been noticed by any chemist before the discovery of nitrous air, I cannot imagine what hypothesis he would have formed for the explanation of them. Whatever it had been, it must have been very wide of the truth; whereas the whole process admits of the easiest explanation imaginable by the help of my observations on the decomposition of nitrous air by the nitrous acid, Vol. III. p. 121.

Nitrous

Nitrous air is actually formed the moment that the ſolution begins, but it is inſtantly decompoſed by the ſtrong ſpirit of nitre in contact with it. By the addition of the phlogiſton contained in the nitrous air, the pale ſpirit of nitre aſſumes an orange colour, and it is then much leſs able to decompoſe the nitrous air; which, therefore, riſes in bubbles through it, and is not decompoſed till it comes to the region of the pale acid lying upon it. But when the whole body of the acid is ſaturated with phlogiſton, then, and not before, the bubbles of nitrous air paſs freely through it, and may be collected.

On this account, it is not eaſy to aſcertain the exact quantity of nitrous air yielded by the ſolution of mercury, and, for the ſame reaſon, of other metals too, in ſtrong ſpirit of nitre; becauſe allowance muſt be made for the quantity that will be imbibed by the acid itſelf, which muſt be ſaturated before any can be collected: whereas, when the acid is much diluted with water, it is not ſo capable of decompoſing this air, and therefore,

therefore, in general, it may be collected from the moment that the ſolution begins.

It is very remarkable, that when copper is diſſolved in pale ſpirit of nitre, even diluted with much water, though the ſolution is evidently the moſt rapid at the firſt, the produce of air is very trifling for a conſiderable time, and the quantity collected increaſes very gradually; whereas when the orange coloured acid is employed, in the ſame diluted ſtate, the nitrous air is collected immediately. and the production is the moſt copious at the firſt.

When I diſſolved a quantity of copper in ſtrong ſpirit of nitre half diluted with water, no air whatever was produced, though the metal was completely diſſolved.

When, in the ſolution of mercury, I uſed the green ſpirit of nitre, inſtead of the pale coloured and ſtrongeſt acid, the phenomena were not materially different from thoſe deſcribed above. The lower part of the acid next to the mercury aſſumed a deeper green, but it never became orange coloured.

SECTION IV.

Of the Changes to which Nitrous Air is ſubject.

BOTH nitrous and inflammable air contain phlogiſton, and, as will be ſeen in its proper place, they probably contain nearly equal quantities in equal bulks; but as their properties are remarkably different, their conſtitution muſt be different alſo; the phlogiſton which enters into the compoſition of them both being combined in them in a very different manner. In ſome caſes nitrous air parts with its phlogiſton more readily than inflammable air, but in other reſpects inflammable air is the more eaſily decompoſed of the two. The phlogiſton of nitrous air immediately quits it on the contact of common air, when it is even quite cold, whereas the phlogiſton of inflammable air will not leave it to join the common air except when it is very hot; but it will be ſeen that inflammable air parts with its phlogiſton to the glaſs of lead

lead in the compofition of flint glafs in circumftances in which nitrous air undergoes no change whatever. I kept a quantity of nitrous air in a tube of flint glafs, hermetically fealed, buried in hot fand, but not fufficient to melt the glafs, twenty days without any fenfible change in the bulk or quality of the air. In the upper part of one tube filled in this manner there was fomething like fmall cryftallizations, but they might poffibly come from a fmall quantity of the quickfilver accidently left in the tube. But whether nitrous air will be decompofed by quickfilver in this ftate of heat and confinement I did not try. Indeed I did not examine whether what I faw were properly cryftallizations, or not.

I kept both nitrous air and inflammable air very hot in contact with quickfilver with liberty to expand, and did not find that either of them underwent any change. A quantity of nitrous air I expofed feveral hours for three days to a degree of heat which kept the quickfilver in a ftate of vapour, the firft and fecond day to the fame quick-

quickſilver, and the third day to freſh quickſilver; but the dimenſions of the air, and its property of affecting common air, continued the ſame. The proceſs is deſcribed in the Introduction.

The addition of ſteam of water to the nitrous air in this ſtate of heat and expanſion made no difference in the reſult of the experiment, though they continued together upon quickſilver more than two hours. The ſmall alteration that I found in the nitrous air might be aſcribed to its having been transferred from the trough of water to the baſon of quickſilver in a bladder. I varied the experiment by confining the nitrous air in a glaſs jar inverted in a pan of water, which I made to boil, in order that the hot ſteam might pervade the whole maſs of the air, which it effectually did, as it appeared by its having expelled a very great part of it. After the proceſs, which continued about an hour, the nitrous air had loſt nothing of its power of diminiſhing common air. On the contrary, it ſeemed, to be rather improved than to have had its virtue impaired.

Since my laſt publication I have obſerved ſeveral more circumſtances relating to the decompoſition of nitrous air, ſome of which are remarkable enough.

In the preface to my third volume I mentioned in general, the quick abſorption of this air by a ſolution of green vitriol, which I had juſt then obſerved. I ſhall here mention the particulars of that obſervation.

Having diſſolved a quantity of green vitriol, and put it into a phial, with its mouth inverted in a baſon of the ſame, and having admitted a quantity of nitrous air to it, I began to agitate the ſolution, in the ſame manner as in the proceſs for impregnating water with fixed air; when I obſerved that the nitrous air, in theſe circumſtances, was abſorbed much more readily than fixed air is by water. I even made a quantity of this ſolution abſorb more than ten times its bulk of nitrous air, without any very ſenſible approach to tion. The ſolution became black by this proceſs; but when a ſmall part of it was viewed by the light of a candle, placed beyond it, it looked red. The taſte of the ſolution was acid, owing, no doubt, to the mixture of nitrous acid, which it had acquired, in conſequence of the decompoſition of the nitrous air.

When this impregnated ſolution was expoſed to the open air, large green cryſtals were formed at the bottom of the veſſel, and all the black colour intirely diſappeared. But when theſe cryſtals were formed at the bottom of a very tall veſſel, they were much blacker, and did not even become green on being expoſed afterwards to the open air, any more than thoſe which I expoſed to nitrous air itſelf on quickſilver.

The changes of colour, and all the phenomena of the cryſtals, were evidently owing to the ſpirit of nitre contained in the nitrous air, and ſet at liberty in its decompoſition. For a few drops of the acid itſelf produced the ſame effects, in all reſpects, on this ſolution.

Conceiving that the principal of theſe phenomena muſt have ariſen from the affinity between nitrous acid and iron, I agitated nitrous air in a natural chalybeate water, when it preſently became of a browniſh colour, which ſeemed to be a confirmation of my ſuppoſition.

I alſo made another experiment in which the nitrous acid might ſhow its affinity to iron in a manner ſomewhat ſimilar to this. I firſt ſaturated a quantity of water with fixed air, then with iron, and afterwards impregnated it with nitrous air. The reſult of this experiment was, that the ſolution aſſumed a colour between green and yellow; but it did not abſorb much more nitrous air than water unimpregnated with fixed air, or with iron, would have done.

The nitrous air which I had hitherto made uſe of in theſe experiments was made from copper, but when I uſed that which was made from iron, which is an ingredient in green vitriol, the effect was not at all different. The ſolution of the vitriol abſorbed nitrous air with the ſame rapidity as

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it did that which was made from copper, and the fubfequent phenomena were alfo, in all refpects, the fame.

I then agitated nitrous air in folutions of blue and white vitriol, the former of which is known to be compofed of copper, and the latter of zinc. The refult was, that the colour of both thefe folutions became prefently very dark, the former changing into a deep green, and the latter into a kind of brown. Not more than between one half and one third of the air (which was about one fourth of the contents of the phial I made ufe of) was abforbed in either of thefe cafes, which is very far fhort of the effect of the folution of green vitriol on the fame kind of air.

It made no difference whether the nitrous air was procured from iron or from copper, in any of thefe experiments. For the folution of green vitriol, as I have obferved, decompofed nitrous air made from iron juft as readily as that which was made from copper; and, on the other hand, the folutions of blue and white vitriol were affected in the very ſame manner by nitrous air made from copper, as by that from iron.

The ſolution of white vitriol depoſited a white and flocculent matter, and then was tranſparent like water; but, being impregnated with nitrous air, it preſently became of as dark a colour as when it had been impregnated before that depoſit was made.

Spirit of nitre dropped into the ſolution of blue or white vitriol made little or no change in their colour.

All the ſolutions of vitriol which had their colour changed by the impregnation with nitrous air recovered it again by expoſure to the common air. This was evidently effected by the eſcape of that phlogiſton, which had contributed to the deepneſs of their colour. To aſcertain this, I filled a phial about three fourths full of the ſolution of green vitriol, made black by the decompoſition of nitrous air, and after about a week, examining the air which had been confined with it, I found it to be ſo much

phlo-

phlogiſticated, that one meaſure of it and one of nitrous air occupied the ſpace of 1.92 meaſures.

Upon the whole, it ſeems that the greater effect of the ſolution of green vitriol in decompoſing nitrous air muſt be owing to the ſtronger affinity between the ſpirit of nitre and iron, than between the ſame acid and copper or zinc.

They ſeem to ſhow, however, that there is little, if any martial earth in nitrous air, at leaſt, that ſuch earth exiſting in nitrous air is not combined with phlogiſton, or in a metallic ſtate; ſince this air is decompoſed by the nitrous acid in it quitting the phlogiſton with which it was already combined, in order to unite itſelf to the iron in the ſolution, at the ſame time that the phlogiſton which entered into the nitrous air contributes to blacken the ſolution. It will, perhaps, however be thought extraordinary, that the nitrous acid ſhould have a ſtronger affinity with iron than the vitriolic, which, on this hypotheſis, it muſt, in this particular caſe, have.

That the ſolution of green vitriol was not blackened by any peculiar affinity that it had with phlogiſton, ſo as to decompoſe the nitrous air by ſeizing upon it ſeemed to be evident from this, that when I made an effervеſcence of iron filings and brimſtone over the ſolution of green vitriol, there was no change of colour produced in it. The ſame was alſo the caſe when this effervеſcence was made over the ſolutions of blue and white vitriol, ſo that though the phlogiſton ſet looſe in this proceſs was imbibed by the air, and phlogiſticated it, theſe ſolutions were not at all affected by it.

This effect of the ſolution of vitriol on nitrous air helps to explain a phenomenon, which I had often obſerved without underſtanding it. When the water in my trough had got impregnated with various metallic ſubſtances, that which was contiguous to the nitrous air, in jars ſtanding in it, would be of a darker colour than the reſt of the water. This muſt have been in conſequence of the affinity between the ſpirit of nitre in the nitrous air and the metallic matter

ter diſſolved in the water, by means of ſome acid that happened to be mixed with it; while the dark colour of the water muſt have been acquired from the phlogiſton of the nitrous air, partly decompoſed by this means. At one time, when the water in my trough was particularly foul, and ſeemed diſpoſed to make a depoſit, I impregnated part of it with nitrous air, and the water, by this means, preſently became of a darker colour than before.

To determine whether the phenomena attending the impregnation of the ſolution of green vitriol with nitrous air depended, in any meaſure, upon the ſeeming *aſtringency* of that ſolution, and of chalybeate waters, I impregnated a quantity of *green tea*, which is alſo ſaid to be aſtringent, with nitrous air, but no ſenſible change of colour was produced in it.

In my former publications I have mentioned a variety of circumſtances in which nitrous air is remarkably diminiſhed, in ſeveral of which it paſſes through a ſtate in which a candle burns in it quite naturally, and ſometimes with a much enlarged flame, and at laſt becomes mere phlogiſticated air. In all theſe proceſſes I took it for granted, (not having examined the air except when it was completely or, at leaſt, very nearly reduced to one of the two ſtates above-mentioned) that the aproximation to its final ſtate of phlogiſticated air was equable, ſo that as ſoon as it began to be diminiſhed, it alſo began to loſe its power of affecting common air. I find, however, that, with reſpect to ſeveral of the cauſes of diminution, and perhaps all of them, the air paſſes very ſuddenly from the ſtate in which it is perfect nitrous air, to the ſtate above-mentioned; but that the term at which this change takes place is various, as ſometimes two thirds, and ſometimes fourteen fifteenths of any quantity on which the experiment is made, will have diſappeared before any ſenſible change can be obſerved in the remainder. I have even ſometimes been inclined to think that its power of affecting

affecting common air has been rather increased than diminished at the beginning of these processes.

I imagine, therefore, that, as soon as either the nitrous acid, or the phlogiston which enters into the composition of nitrous air, is seized upon by any substance which has a stronger affinity with either of them than they have with each other, so much of the other principle as was combined with it is precipitated, so that the air which remains is not at all altered from what it was, at least for a considerable time. It will appear, however, that the slower the process is, the greater quantity of nitrous air will be preserved in the state of phlogisticated air, and the quicker the process, the farther it will proceed before this change takes place.

I entertained the first suspicion of my having been mistaken in my former opinion (though it is not an opinion that I believe is directly expressed in any of my former publications) when I was examining some nitrous air in which I had confined

fined a fowl, in order to preſerve it as long as poſſible from putrefaction. For though this air was greatly diminiſhed in quantity, it affected common air quite as much as the beſt nitrous air I had ever tried.

Being deſirous of aſcertaining this fact with abſolute certainty, with reſpect to ſome one cauſe of the diminution of nitrous air, I placed a pot of *iron filings and brimſtone* in a jar of nitrous air, and let it remain there a whole day, keeping it generally warm, near the fire, the ingredients not being good of their kind, and not diſpoſed to ferment. As the diminution proceeded, I kept taking from it ſmall portions of the air, by introducing into it a ſmall jar full of water; which, being emptied within the jar, I withdrew, filled with the air from within it. Doing this occaſionally, I obſerved no change of the quality of the air when it was reduced to one third of its original bulk, for it retained its full power of diminiſhing common air.

The next day I found it diminiſhed to one fourth of the whole, and then a candle burned

burned in it in a manner not to be diſtinguiſhed from the burning of a candle in common air. But it was not common air; for it was not at all diminiſhed by freſh nitrous air, and it affected common air ſo little, that one meaſure of it and one of common air occupied the ſpace of 1.85 meaſures. It had not acquired the peculiar property of fixed air, for it did not make lime water in the leaſt degree turbid, and it bore conſiderable agitation in water without being much diminiſhed.

The diminution of nitrous air by means of *ſpirit of nitre* is effected in the ſame manner; and as this diminution was made more quickly, on this account, perhaps, it proceeded much farther before I could perceive any change in it. In one experiment of this kind, I thought the change took place when the air was diminiſhed to one twelfth of the whole, but in another caſe there was no change till it was reduced to between one twelfth and an eighteenth part, when it was completely phlogiſticated. In this mode of diminution I was

not

not able to find it in that ſtate in which a candle could burn in it. At another time when exactly one fifteenth of the whole remained, it affected common air manifeſtly leſs than freſh nitrous air; but here again, when only one eighteenth remained it had loſt all its peculiar property.

The diminution of nitrous air by the *ſolution of green vitriol* is effected according to the ſame rule. I decompoſed nitrous air by expoſing it to be abſorbed by the ſolution of green vitriol till about one fourth of the original quantity remained, but it affected common air as much as it had done before any part of it was abſorbed.

Such, alſo, is the manner in which nitrous air is diminiſhed in a *bladder*, as it is deſcribed in my third volume, p. 151. Nitrous air reduced in this manner from ten ounce meaſures to two and an half, was ſo much altered, that one meaſure of it and one of common air occupied the ſpace of 1.75 meaſures. It then extinguiſhed a candle without any appearance of a blue flame. When a little more of it was abſorbed by the

the ſame proceſs, I found the remainder all phlogiſticated air, not affecting common air in the leaſt. Till the nitrous air was reduced, in this manner, to very near one fourth it continued unchanged.

If, however, common air be mixed with nitrous air, by which means it becomes in part phlogiſticated air, the ſpirit of nitre will abſorb the ſuperfluous nitrous air only, and conſequently leave the remainder more diluted with phlogiſticated air. I put a meaſure and a half of common air to two meaſures of nitrous air, ſo that one meaſure of this mixture and one of common air occupied the ſpace of 1.36 meaſures. I then put ſome ſpirit of nitre to the mixture, and when it had abſorbed one-third of it, one meaſure of it and one of common air occupied the ſpace of 1.8 meaſures.

I alſo introduced a piece of hot *charcoal* into a phial of nitrous air by which means one half of it was abſorbed, and found that the remainder had not loſt its power of diminiſhing common air in any ſenſible

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degree. The abſorption of all kinds of air by *charcoal* is a very capital diſcovery of the Abbé Fontana, which he has been ſo obliging as to give me leave to mention.

When nitrous air has been kept a long time in water, it is known to be diminiſhed, and in this caſe I ſuſpect that it loſes its virtue gradually, being impaired from the firſt. I have found, however, that by long keeping in perfectly ſtagnant water, and without any change, except to ſupply the waſte by evaporation, it came to the ſtate of phlogiſticated air; but by what ſteps in the progreſs I omitted to obſerve, having taken it for granted that this was always equable.

On the 11th of November 1773, I filled two quart bottles with freſh made nitrous air, one from iron, and the other from copper, and then ſet them aſide, with their necks immerſed in jars of water, and never agitated the air or the water in contact with it, only ſupplying the jars with freſh water as I perceived it was wanted. On the 29th of September 1778, I examined the ſtate of theſe thefe bottles of air, and found as follows. Of that which had been made from iron about one half was abforbed, and of that from the copper about a third; but both of them were equally and perfectly phlogifticated, making no effervefcence with common air, and extinguifhing a candle. That which had been made from copper did not make lime water turbid, and the fame, I doubt not, would have been the cafe with the other, if it had been tried. I did this from a fufpicion, that, fince fixed air may be compofed from the nitrous acid, nitrous air, in fome of its changes, might, in part, affume that form. I had not given much attention to thefe bottles of air, but I do not think they had been at all diminifhed the laft year, or the laft year and half.

I was not able to catch the inflammable ftate, as I may call it, of nitrous air in diminifhing it by the *electric fpark*, nor did I attend to the intermediate ftate of it; but I have made this experiment more at large than I had done before, and the particulars

ticulars may be worth reciting. I nearly filled a phial, containing about ſix ounce meaſures of nitrous air, confined by quickſilver; and taking the electric ſpark within it, it was in about an hour diminiſhed about one half, but after that very little. The quickſilver was much corroded, and a candle went out in the remainder of the air. The diminution of common air by the electric ſpark requires a good deal of time, but this proceſs goes on very rapidly with nitrous air. I repeated the experiment in a tube a quarter of an inch in diameter, receiving the ſpark upon water tinged blue with the juice of turnſole; and the diminution was ſo quick, that the motion of the water up the tube was conſtantly ſenſible to the eye. The water was deeply and permanently red.

Mr. *Bewly's Pyrophorus* alſo decompoſes nitrous air, and preſently reduces it to the ſtate of phlogiſticated air. Having put a quantity of it into a glaſs jar ſtanding inverted in quickſilver, I introduced ſome nitrous air to it, when the pyrophorus

became

became inſtantly red hot. What remained of the nitrous air had no effect on common air, and extinguiſhed a candle. All this change was effected at once. For though the nitrous air continued in the jar a day and two nights after it had been admitted to the pyrophorus, there was no farther change in its dimenſions.

The *willow plant*, as I ſhall obſerve, abſorbs nitrous air as well as every other kind of air. What were the intermediate ſtates of it I did not note, but when the air was reduced to one tenth of its bulk, I found it to be mere phlogiſticated air.

SECTION V.

Of the Impregnation of Water with the Vapour of nitrous Acid.

I HAVE obſerved that the conſequence of impregnating water with the vapour that eſcapes from ſpirit of nitre is making it ſparkle, with the ſpontaneous production of nitrous air. This ſeems to prove that, unleſs there be earth in all water, there cannot be any earth neceſſarily contained in nitrous air. But at the time of my former publication I had always produced this appearance by throwing into the water the red nitrous vapour from a violent efferveſcence of ſpirit of nitre and biſmuth; and in this violent efferveſcence it was poſſible that ſome of the earth of the metal might be carried over, as ſome of the water evidently was. I was, therefore, now careful to avoid this objection, which I did by expoſing a phial of pure nitrous acid to nitrous air over the pureſt diſtilled water. This I did by means of a tube with a ground ſtopper at each end. For by ſtopping and unſtopping them alternately, I could eaſily manage ſo as to place the phial of ſpirit of nitre, ſupported by a thin glaſs tube, very near the top of the veſſel, then fill it quite to the edge of the veſſel with water, and after that diſplace the water by introducing nitrous air. As the nitrous air was abſorbed I introduced more, by means of a bladder previouſly filled with it. The quantity of common air above the ſpirit of nitre was quite trifling in proportion to the bulk of the tube.

In theſe circumſtances I obſerved that when the nitrous acid became blue, and hardly before, the water next to it began to emit bubbles of air. To the formation of this air (which was doubtleſs nitrous air) nothing could contribute but the effluvia of the nitrous acid, and ſomething that the water itſelf might furniſh; and this water had been ſlowly and carefully diſtilled in glaſs veſſels.

The quantity of water uſed in this experiment was about four ounce meaſures, and the quantity of nitrous air abſorbed was about fifteen or twenty ounce meaſures; the circumſtances of the experiment being ſuch that very little more could have been abſorbed without changing the acid. I then carefully diſtilled the water, which had imbibed whatever had been precipitated from the decompoſed nitrous air, and found a pretty large earthy ſediment, covering a ſpace at the bottom of a retort of about an inch and a quarter in diameter, beſides having made a great number of white ſpecks at a conſiderable diſtance from that central ſpot. This matter was generally white, but where it was thickeſt it was ſlightly orange coloured. Spirit of ſalt diſſolved the whole of this earthy matter, and became of a deep orange colour in conſequence of it. This might ſeem to be earth which had been precipitated from the nitrous air, and perhaps ſome of it might have been thus produced; but when I afterwards evaporated to dryneſs the ſame quantity of the ſame diſtilled water I found a larger earthy ſediment than I had expected; and though I think not ſo much as that above deſcribed, yet enough to make me heſitate in drawing a general concluſion from it.

SECTION VI.

Attempts to preſerve animal Subſtances in nitrous Air.

IT was among my earlieſt obſervations on nitrous air, that animal ſubſtances would not putrefy in it. I have ſince my laſt publication made a few experiments, in order to aſcertain whether it be poſſible to derive any advantage from this property of nitrous air for *culinary purpoſes*. But I cannot ſay that my obſervations have been very favourable to it in this reſpect. Nitrous air will, indeed, preſerve fleſh meat from putrefaction; but after long keeping in this manner it becomes very offenſive, both to the noſtrils, and the palate, though the ſmell is not altogether that of putrefaction; and indeed the ſubſtance continuing quite firm, it could not be properly putrid. Though theſe experiments were not quite fair, becauſe the nitrous air had not been renewed ſo often as it ought to have been, ſeveral of the phenomena may be worth mentioning.

On the 28th of April 1777, I put two pigeons into two jars of nitrous air, juſt wide enough to contain them, with about as much nitrous air in the jars, as the bulk of the pigeons. From this time till the 4th of June following I had renewed the nitrous air but once, and then, taking them out, I found them both free from all ſmell of putrefaction. One of them was broiled, when the fleſh was found to be ſweet, but it had not the natural taſte of the pigeon, and was, on the whole, unpleaſant. The fleſh was quite red throughout, and a little harder than that of a pigeon generally is. The water contained in the cups, in which the jars with the pigeons had ſtood, had generally been very offenſive, ſo that it ſhould ſeem that the putrid effluvium (containing, probably, much phlogiſton, and perhaps the moſt nutritive part of the fleſh) had paſſed through the nitrous air, and the water, into the ſurrounding atmoſphere.

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I replaced the pigeon that was not uſed, and let it remain, along with two others which had been kept the ſame time, till the 13th of September following, in all, near ſix months, or the whole ſummer ſeaſon; but I had not been careful to change the air very often, though I did it two days before I took them out the laſt time. The pigeons had now certainly a very bad ſmell, though their fleſh was firm, and ſo were even the bowels of one of them which had not been drawn. When they were dreſſed, they were much more offenſive, and had a ſtrong ſmell of putrefaction or ſomething very much reſembling it. The fleſh was red throughout, ſtill firm, and excluſive of the ſmell, had little or no taſte. My friend, Mr. Magellan, who was with me at the preparation of them, had not ſo bad an opinion of this piece of cookery as I had.

On the 10th of May I put into a jar of nitrous air a large wood-pigeon; and taking it out on the 18th of June following, obſerved that it had a ſtrong and offenſive ſmell, but the fleſh was perfectly firm. Though a very great part of the air had been abſorbed, and during the fortnight preceding the examination it had not been ſupplied with freſh air, as it had been occaſionally before, the air to which it had been expoſed all that time diminiſhed common air quite as much as freſh made nitrous air. It was this obſervation that gave me the firſt ſuſpicion of the manner in which nitrous air is diminiſhed in this and in other proceſſes. Having replaced the pigeon in the jar, I found on the 7th of Auguſt following, that the air was but ſlightly nitrous, and on the 22d of the ſame month it was mere phlogiſticated air. After this I neglected to attend to it, and at laſt threw it away. Whether, in this proceſs, the nitrous air ever comes into a ſtate in which a candle will burn in it, or not, I cannot tell. The experiment is a very unpleaſant one, and I ſhall hardly repeat it.

In all theſe caſes the fleſh was kept a long time, *viz.* through the ſix ſummer months; and though nitrous air failed to preſerve preferve meat in a ftate fit for eating fo very long, it may poffibly anfwer the purpofe for a few days tolerably well, as it will certainly reftore meat that has begun to turn putrid. One trial of this kind I did make.

On the 14th of June 1777, I took a fowl which had been killed a week, and which had been purpofely kept till it was offenfive; and putting it into a jar of nitrous air, obferved that the air began immediately to be abforbed, and on the 16th I took the fowl out, when it had no fmell of putrefaction at all; but when it was boiled, though myfelf and feveral other perfons tafted of it, and perceived nothing difagreeable in the tafte itfelf, we were difgufted with a faint fmell that came from the body of the fowl, when we held it to our noftrils. Perhaps it had not been expofed to the nitrous air quite long enough.

Though part of this air had been abforbed, the remainder diminifhed common air quite as much as any frefh made nitrous air.

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On the ſubject of this ſection I ſhall obſerve that Dr. Millman having been ſo obliging as to inform me that he had found that *bile* is prevented from becoming putrid much longer by being impregnated with fixed air, than it could otherwiſe be; I was deſirous of trying what effect the impregnation with nitrous air would have upon it. Accordingly, on the 19th of February 1777, I impregnated a quantity of ox bile, with which he ſupplied me, with nitrous air; when, from being viſcid, it preſently became limpid like water, and aſſumed a browniſh hue, without depoſiting any thing that I could perceive. This bile continued perfectly ſweet till the 20th of March following, when it was packed up, along with other things, and removed from London into the country. Examining it ſome time afterwards, I found it had contracted a ſmell of putrefaction, and on the 23d of April, it was quite putrid. The ſame brown colour continued, but it had depoſited ſomething of a whitiſh colour.

SECTION VII.

Miſcellaneous Experiments relating to nitrous Air.

I. OLIVE *oil,* by which a quantity of nitrous air had been confined in a phial ſeveral months, had abſorbed almoſt the whole of it, and that part of the oil which was contiguous to the air was coagulated in lumps, as if it had been frozen, and remained a long time at the top of the oil. But afterwards, being looſened, I ſuppoſe, by the warmth of the weather, it all ſunk to the bottom, as the ice of oil always does.

II. I rather ſuſpect that when nitrous air is mixed with common air, in a greater proportion than is requiſite to the complete ſaturation of the common air with phlogiſton, the ſuperfluous nitrous air is more diſpoſed to be abſorbed by water than pure nitrous air. It appears, however, that, in no great length of time, ſuch mixtures are brought to the ſame dimenſions as if only half

half the quantity of nitrous air had been mixed with the common air. This, I think, may be inferred from an experiment which I made to try the difference between *old* and *freſh made* nitrous air, both having been made in the ſame manner, and, I believe, having been originally of equal ſtrength.

October 25, 1777, I mixed equal quantities of the ſame common air with equal quantities of the old and freſh made nitrous air. What ſpace they occupied at that time, and in ſeveral ſubſequent periods, is repreſented at one view, as follows:

	With the old nitrous air.	With the new.
Oct. 27, 1777.	1.22	1.05
Nov. 10,	1.07	0.93
24,	0.96	0.86
Feb. 2, 1778.	0.84	0.8

The laſt is one fifth leſs than the original bulk of the common air, and conſequently very near to the utmoſt limit of the diminution of common air by any proper phlogiſtic proceſs. An accident prevented my obſerving this progreſs any farther.

III. I found, very unexpectedly, that a confiderable difference would be made in the dimenfions of the mixture of air by a circumftance in the *manner of mixing* them that one would not readily fufpect, and I am not able to account for it. My ufual method, as I have obferved in the Introduction, has been to mix equal meafures of nitrous and common air in a low jar, and then to transfer the air into a graduated tube, three or four feet long. What I obferved is, that I could make a difference of five hundred parts of a meafure by making the air run up the long tube quickly or flowly. The more flowly it afcended, the lefs fpace it occupied. To afcertain whether it depended merely upon the two kinds of air being fo much longer together in the wider veffel, or in the funnel through which it was poured into the tube, I made the mixtures over night, and transferred them into the graduated tube the next morning, but I ftill found the fame difference, depending upon the circumftance above-mentioned.

SECTION VIII.

Of the Colour of the Marine Acid.

ALL the chemiſts, as far as I can find, who have written on the ſubject of the marine acid, ſpeak of its *colour*, as of a thing eſſential to it, and never fail to deſcribe this as a neceſſary part of its definition: "Thus Mr. Macquer, in his *Dictionary*, ſays, that this acid differs from the vitriolic in having *ſmell* and *colour*." He alſo ſays, it differs from the nitrous acid by its colour, "which is more yellow and leſs red."

In the experiments of which I gave an account, in my third volume, I gave a good deal of attention to this ſubject, but at that time I had not been able to aſcertain on what it is that the colour of this acid depends. Sometimes, I there obſerved, I had procured it quite colourleſs, eſpecially when I made it by impregnating water with marine acid air, but at other times I was not able, though I endeavoured to do it it, to procure it without colour. I have ſince, however, perfectly ſatisfied myſelf with reſpect to the colour of this acid, and can at any time make it as colourleſs as water itſelf, the colour always coming from ſome impregnation, generally, if not always, of ſome *earthy matter*; with almoſt every thing of which kind it unites, and from which it generally takes ſome colour or other. I can alſo inſtantly diſcharge any colour that this acid has acquired, and reſtore it again at pleaſure, as will appear in the courſe of theſe obſervations.

As I always make my own ſpirit of ſalt, as well as my ſpirit of nitre, and was ſatisfied, from my former obſervations, that colour is not eſſential to this acid, any more than to the nitrous, or the vitriolic; on the firſt of Auguſt 1777, having occaſion for a quantity of ſpirit of ſalt, I was determined to make the diſtillation with all the attention that I could give to it, taking the produce at different times, which is my general cuſtom, and which has been the occaſion of my making a variety of im-

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portant obſervations. I alſo received the ſuperfluous vapour, or marine acid air, with the ſame precautions, and in the ſame manner. The apparatus was nearly the ſame with that of which a drawing is given in the plate to my third volume, Fig. 4. The retort only being much larger, and uſing phials with water inſtead of the cup *g*. In this proceſs alſo I ſeldom make uſe of any adopter.

Every thing being thus prepared, and having luted the veſſels with a mixture of clay and fine ſand, I began the diſtillation; and obſerved that the firſt produce was ſtraw coloured, as uſual; but all that came afterwards was quite colourleſs, like water. Alſo, all the impregnations of the water with the ſuperfluous vapour were colourleſs. But the heat happening to abate towards the end of the proceſs, a quantity of water ruſhed ſuddenly from the phial that received the impregnation, through the receiver, into the phial that contained the diſtilled acid; when all the acid that was in it, which was then quite colourleſs, imme-

immediately aſſumed as deep a ſtraw colour, as that of the firſt produce of the diſtillation.

This proceſs might have been ſufficient to explain to me the whole myſtery of the colouring of the ſpirit of ſalt; but it did not, and all the real advantage I gained by it was having in my poſſeſſion a large quantity of pure colourleſs ſpirit of ſalt, to which I might endeavour to give colour in future experiments. For all the hypotheſis that occurred to me from conſidering the phenomena of this proceſs was, that the colouring of this acid, as in moſt other caſes, and eſpecially in ſpirit of nitre, was owing to *heat*, or *phlogiſton*; ſo that I was miſled by the general maxims of the chemiſts, and alſo by the analogy of the two acids, and, indeed, that of the vitriolic acid alſo, which is known to acquire its black colour from ſubſtances containing phlogiſton.

Thus I conſidered the colour of the firſt produce of ſpirit of ſalt, in the above-mentioned proceſs, as ſimilar to the uſual colour of the firſt produce in the diſtillation of ſpirit of nitre, *viz.* to ſome unobſerved phlogiſtic matter in the materials; and I conſidered the deep ſtraw colour at the laſt, as occaſioned, likewiſe, by ſome phlogiſtic matter driven into the veſſel by the ſudden ruſhing in of the water. Beſides, I had more than once found ſpirit of nitre to become inſtantly of a deep green by a ſimilar ruſhing of water into the recipient.

Conceiving that it muſt be phlogiſton that gave colour to this acid; as well as to the nitrous acid and the vitriolic, I imagined I had nothing to do but to diſcover the proper mode of combining them; and I made trial of ſeveral things for that purpoſe, as putting into the colourleſs acid bits of charcoal, quenching hot charcoal in it, and mixing with it various other ſubſtances containing phlogiſton, both hot and cold, but all without any effect.

As I had given colour to ſpirit of nitre by merely heating it in glaſs tubes hermetically ſealed, I ſubmitted the ſpirit of ſalt to the ſame trial; and for ſome time imagined

imagined that I had ſucceeded. For, in ſeveral inſtances, the ſpirit of ſalt did become coloured in theſe circumſtances.

About half an ounce meaſure of colourleſs ſpirit of ſalt being confined in a glaſs tube an inch in diameter, and three feet long, hermetically ſealed, on being expoſed to heat, preſently aſſumed the deepeſt uſual colour of ſpirit of ſalt. Suſpecting that there might have been ſome unperceived bit of ſtraw, or ſome ſuch thing in the large tube, I took a ſmall one that was perfectly clean; and preparing it in the ſame manner, I expoſed it to the heat of a common fire, and with the very ſame reſult. The acid had acquired a perfect ſtraw colour.

But I was more confirmed in my opinion that it was heat, or phlogiſton, or both, that produced this effect by finding that I got a peculiarly deep ſtraw colour when I had incloſed the ſpirit of ſalt in a tube in which ſome oil had been before expoſed to heat in the ſame manner, and to which a little of it adhered: and, what I had not much attended to before, I now obſerved that the acid retained this ſtraw colour when it was quite cold. But, notwithſtanding theſe promiſing appearances, my hypotheſis was totally overturned by finding, a day or two afterwards, that when I had expoſed two glaſs tubes, in all reſpects, as nearly as I could judge, alike, containing the ſame colourleſs ſpirit of ſalt, to the ſame fire, and the ſame length of time, only one of them acquired the ſtraw colour, while the other continued colourleſs, as at firſt. I examined both theſe tubes with the greateſt attention, but could not diſcover any cauſe of this difference. There was indeed, more of the earthy matter, of which I ſhall treat preſently, in the tube in which the acid was coloured, but that in which the acid continued colourleſs had a ſmall crack in it, out of which ſome of the acid had oozed, ſo that I did not attribute this difference of colour to that circumſtance.

At length, on the 6th of September, I diſcovered, by the mereſt accident, the whole

whole myſtery of what I had been ſo long, and ſo intently inveſtigating. For, having ſome other uſe for the phial which contained the ſpirit of ſalt, I poured it into another phial, in which there had formerly been ſome iron filings and water, and the ſides of which had a ſlight incruſtation of ochre, which is known to give to glaſs a tinge that is not eaſily got out: but the moment that the colourleſs ſpirit of ſalt touched this red incruſtation, it became of a deep ſtraw colour, and the phial where-ever it had been touched by the acid, was perfectly clean.

After this it was impoſſible not to conclude that the colour of ſpirit of ſalt is not owing to phlogiſtic matter, like the colour of oil of vitriol, or that of ſpirit of nitre, but to an impregnation of ſome earthy matter, with which it is known readily to unite; and farther obſervations preſently placed this hypotheſis beyond all poſſible doubt. I was now alſo ſatisfied, that the firſt produce of ſpirit of ſalt, in the proceſs above-mentioned, muſt have touched fome of the clay, or fand, with which the veffels had been luted, and that the water, in its violently rufhing into the receiver, muft have met with more of it, though at that time, fufpecting nothing of this, I did not perceive it.

SECTION IX.

Of the Impregnation of Marine Acid with various earthy Subftances.

HAVING now difcovered the power of the marine acid to diffolve earths, I was defirous of examining the circumftances attending various folutions of this kind, both with refpect to the earths themfelves, and the colour of the faturated acid.

Spirit of falt diffolves a great quantity of *ruft of iron* with effervefcence, but not with much heat. The mixture was of a very deep brown, and what was not diffolved was of a dirty blackifh colour. But poffibly this might be owing to the ruft of iron not being perfectly free from all foreign

foreign matters. The ſpirit of ſalt thus ſaturated with the ruſt of iron diſſolved iron filings, and produced inflammable air; after which it was green. Having ſaturated a quantity of ſpirit of ſalt with the ruſt of iron, I evaporated it to dryneſs, when all the fluid part was diſperſed in colourleſs fumes, and the ochre was left behind, and was rediſſolved by freſh ſpirit of ſalt. I would obſerve, by the way, that ſpirit of ſalt is of excellent uſe to clean glaſs veſſels tinged with the ruſt of iron, and many other matters. This may poſſibly have been known to others. To me the obſervation was caſual, but of great value.

This acid diſſolved a large quantity of *flowers of zinc* with great heat and efferveſcence. During the ſolution the acid became of a turbid black colour, but when it ſtood to ſubſide, the black matter floating in it was depoſited upon a mixture of black and white matter at the bottom of the phial, and the ſaturated acid was quite colourleſs, exactly like water. Alſo when I put flowers of zinc to ſpirit of ſalt deeply coloured with the ruſt of iron, the acid became colourleſs again.

Minium became white by the affuſion of the ſpirit of ſalt, which acquired from it a beautiful yellow colour. A great quantity of it was diſſolved, though more of it remained undiſſolved than of the flowers of zinc. When the red colour of the minium was quite diſcharged, freſh ſpirit of ſalt, though it diſſolved, and became ſaturated with the white minium, acquired no colour from it.

When I had frequently waſhed a large quantity of minium in ſpirit of ſalt, (though not till no more of it would have been diſſolved) I put it into a green glaſs retort, and expoſing it to as much heat as the glaſs would bear, I got from it hardly any fixed air, but about as much dephlogiſticated air as I imagine it would have yielded before any ſpirit of ſalt had been applied to it. It ſeems, therefore, that the ſpirit of ſalt diſlodges from the minium all the fixed air it contains, but has no power of affecting its property of yielding dephlogiſticated gifticated air. The matter melted into a red fluid fubftance, which, when cold, expanded, and broke the retort. This refiduum gave a yellow tinge to fpirit of falt.

Spirit of falt diffolved a great quantity of *red precipitate*, with great heat, but without effervefcence. During the folution the acid was of a turbid white colour, and the precipitate is generally black, though fome parts of it continued red till they were quite diffolved. But what remained undiffolved at the laft was all black. After it had fubfided, all the opake matter was depofited, and the acid was beautifully tranfparent.

This acid diffolves a great quantity of *lapis caliminaris*, but not the whole of any part of it. The folution is made without heat, and it leaves no colour whatever in the fpirit of falt.

Spirit of falt had no effect whatever on *crude antimony*, on *wolfram*, calcined or uncalcined, or on *white arfenic*. It is not affected by *vermilion* immediately; but in time

time it acquires from it a delicate yellow colour. It has alſo no ſenſible immediate effect on the black powder into which mercury is converted; but when lead is mixed with it, it, in time, acquires a deep orange colour from it. This muſt be produced by its ſeparating the calx of lead from the ſuperphlogiſticated mercury, with which it is mixed.

All the above-mentioned ſolutions are thoſe of metallic earths, or other metallic matters, in ſpirit of ſalt. The following obſervations relate to the ſolution of earthy ſubſtances of a different kind in the ſame acid.

Colourleſs ſpirit of ſalt diſſolves completely a great quantity of very white *lime*, and is then of a ſtraw colour; and the ſame was the effect of the ſolution of a pure lime from oyſter ſhells. It alſo diſſolved as much lime of a common ſort, and was then of a true orange colour. But this ſeemed to be owing to a browniſh matter in the lime, which was probably ſome earth of iron that was contained in it. At the ſame time

time I obſerved that lime was not ſenſibly affected either by oil of vitriol, or ſpirit of nitre.

This acid diſſolves a large quantity of calcined *magneſia*, and is then of a ſtraw colour.

It does not ſenſibly affect *glaſs*, but when it was confined in a glaſs tube hermetically ſealed, with a quantity of *pounded glaſs*, and expoſed to a boiling heat, the glaſs ſeemed to be a good deal diſſolved, and the acid became of a ſtraw colour.

From *pipe clay* ſpirit of ſalt acquires a delicate yellow colour.

Wood *aſhes*, out of which air had been expelled by heat, were diſſolved in ſpirit of ſalt, and became black, but the colour of the acid was not changed.

The following ſubſtances were not ſenſibly affected by ſpirit of ſalt, *viz.* plaiſter of Paris, ſteatites, flint, zeolyte, fluor cruſt, Moſcovy talck, cream of tartar, ſedative ſalt, or borax. It had alſo no effect on the black matter that remains in the retort after the proceſs for making ether.

It

It appears to me that it might be of conſiderable importance to the advancement of chemical knowledge to go through with the examination of all earthy ſubſtances in this manner, aſcertaining whether they be ſoluble or inſoluble in ſpirit of ſalt, and noting all the phenomena reſpecting either the earths themſelves, or the acid, and comparing the reſults with the effects of other acids, &c. on the ſame earths. If any thing of this kind be done, at leaſt to much extent, it is unknown to me.

SECTION X.

Of the Effect of a continued Heat on Spirit of Salt in Glaſs Tubes hermetically ſealed.

HAVING made theſe ſolutions of earthy matters in ſpirit of ſalt, I expoſed ſeveral of the ſaturated ſolutions, and other things into which the marine acid enters to a continued heat, and noted ſeveral remarkable effects of that proceſs. But before

before I relate any of them, it will be proper to give an account of the treating of pure ſpirit of ſalt in the ſame manner, beſides what has been ſaid of this proceſs in a former ſection. In general, the ſpirit of ſalt, expoſed to heat in glaſs tubes hermetically ſealed, is enabled to do what it is incapable of in other circumſtances, viz. to diſſolve the glaſs itſelf, and more eaſily to ſeize upon metallic matters, as the calx of lead, and therewith to form a concrete ſubſtance, into which the acid itſelf enters.

On the 30th of Auguſt 1777, I expoſed to a boiling heat, in a glaſs tube about four feet long, and one third of an inch in diameter, as much ſpirit of ſalt as meaſured in the tube about an inch in length, and kept it boiling about two hours. After this the acid was ſtill quite tranſparent, and the quantity not ſenſibly changed, but I obſerved that there was formed, as it cooled, a number of ſmall cryſtals, perfectly white, at the bottom of the acid, and adhering to the ſides of the tube. When I melted the end of the tube with a blow pipe, the preſſure of the atmoſphere forced the glaſs inwards. From this it was evident that there had been a decreaſe of elaſtic matter within the glaſs, which muſt have been produced by the incorporation of the acid vapour in the cryſtals that I have mentioned: for had it been a mere abraſion of the glaſs, beſides that it would have been a powdery ſubſtance, and not in a concrete maſs, the acid vapour would have been ſet looſe by the heat, and therefore would have preſſed the ſoftened glaſs outwards.

Making uſe of a tube an inch wide, and putting into it half an ounce meaſure of tranſparent ſpirit of ſalt, the cryſtals began to be formed in about an hour above the ſurface of the acid, and coated the tube about three inches, but all of it on the upper ſide, the tube having been placed in an inclined poſition.

When I expoſed to the ſame heat the two tubes mentioned before, in one of which the acid was coloured and the other not, I obſerved that more of this ſolid matter

was

was formed in the former than in the latter, the acid having become coloured by diſſolving the glaſs.

When any of theſe tubes happened to be cracked in the proceſs, which was frequently the caſe, there was always a conſiderable incruſtation formed on the outſide of the glaſs, ſpreading from the crack, out of which the acid had eſcaped.

Having obſerved that, in proportion as this earthy, or rather ſaline matter was formed, the acid was diminiſhed; to try whether there was any difference in the acid that remained from what it had been, I took it out of the tube in which it had been expoſed to the heat, and expoſed it again in a freſh tube; but I found that more ſaline matter was formed in this tube, exactly as in the former. I repeated the ſame proceſs on the acid that remained in the ſecond tube, by putting it into a third, when more ſaline matter was produced; and this I repeated till very little liquid acid remained, though the tube broke, and a little

little remaining acid eſcaped, before I had quite finiſhed my proceſs upon it.

At length, however, I completely effected what I had been in purſuit of. For I expoſed a quantity of acid in this manner till nothing liquid remained in the tube. This acid was diſtilled water impregnated with marine acid air, the quantity was about half an inch in length, in a glaſs tube a quarter of an inch in diameter. The lower part of the tube had a thick incruſtation of white matter, and no more moiſture remained within it, than what adhered to the ſides of the tube, and would not run down it.

Though the acid continued to the laſt to diſſolve the glaſs, it was evidently weakened by the continuance of this proceſs, ſo that though both the marine acid air, and the water with which it was incorporated, had entered into the compoſition of the ſaline matter formed within the tube, there was in it more of the acid than of the water. Having extracted a conſiderable quantity of this ſaline matter from one of theſe tubes,

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I took out the remaining acid, and from a given meaſure of it, diluted with water, and bits of iron, I got three ounce meaſures of inflammable air; whereas from the ſame quantity of the ſame original ſpirit of ſalt I got, in the ſame circumſtances 4.1 ounce meaſures. Allowance, however, muſt be made for the vapour that had eſaped in pouring the acid into the tube, and out of it again.

In order to get a quantity of this ſaline matter, I kept a large tube with about an ounce meaſure of ſpirit of ſalt in the ſand furnace near three months, and ſucceeded pretty well. It was all formed in or near the ſurface of the acid. The heat had been very moderate. For great care muſt be taken leſt the glaſs ſhould burſt in this proceſs. It ſeems, however, that when the heat is more conſiderable, the hotter acid may diſſolve the concreted ſaline matter that it comes into contact with, as appears in the following experiment.

Having expoſed 2dwts. of colourleſs ſpirit of ſalt, in a long tube, about one third of an inch in diameter, the tube was preſently incruſted about the length of nine inches with the ſaline matter, but very thin; and I obſerved that there was none of it within an inch of the ſurface of the fluid. Then making it boil more violently, I obſerved that whenever the hot acid reached the incruſtation, it diſſolved it, and waſhed the glaſs quite clean. By this means all the incruſtation was preſently waſhed off, and while the acid continued to boil, it did not appear again.

The reaſon why this incruſtation was generally made at, or rather above the ſurface of the boiling acid, ſeems to be, that the acid was there the moſt concentrated, on its expulſion from the water; and this made a ſtriking difference between theſe experiments, made with ſpirit of ſalt, and ſome which I made with water in the ſame manner. For when I bent the tubes at each end, and expelled the liquors by heat from one end of the tubes to the other alternately, I obſerved that with the ſpirit of ſalt the incruſtations were always made above the ſurface of the boiling liquor; whereas, in the tubes which contained water only, the incruſtations were always made at the place from which the water laſt evaporated.

That the ſpirit of ſalt, in theſe experiments, diſſolves the glaſs, and eſpecially the lead that was in it, appeared from the following obſervation, which was firſt made by Mr. Magellan, who happened to be with me at the time. We had waſhed a quantity of this earthy, or ſaline matter, in diſtilled water; when he obſerved that the water had the taſte of *ſaccharum Saturni*, and when the water that had been uſed in this manner was mixed with pump water, it turned it white, a manifeſt proof of its containing a ſolution of lead.

Spirit of ſalt not only diſſolved this matter when it was hot, but alſo a conſiderable proportion of it when it was cold. When I had waſhed a quantity of it frequently with diſtilled water, till it was quite inſipid, it was not at all affected by oil of vitriol, or ſpirit of nitre; but when I had poured upon it ſome ſpirit of ſalt, and let them continue together a whole day, three grains of it were reduced to a grain and a half; ſo that half of it was diſſolved by the ſpirit of ſalt, and the acid acquired a deep orange colour. As all the ſaline matter had been waſhed out of this ſubſtance by the water, what remained muſt have been the earth of the glaſs reduced to a powdery form, proper for the ſpirit of ſalt to act upon.

There was an incruſtation of whitiſh matter when I made theſe experiments in the green or the black bottle glaſs, which has no lead in it, but it is manifeſtly of a different nature from that which is formed in the flint glaſs. The quantity is much leſs, and it differs from the other in ſeveral reſpects. When I dipped a large piece of a glaſs tube, completely covered with this incruſtation, and which was perfectly white, in freſh ſpirit of ſalt, it preſently diſappeared, as if the acid had diſſolved it all at once; and the incruſtation ſeemed to imbibe the acid, as a wet ſpunge imbibes water:

water: for when the lower part of it was dipped in the acid, it prefently afcended, and moiftened the upper part. But when I took this tube out of the acid, and dried it in the open air, the incruftation re-appeared, exactly as at firft. Alfo the acid in which it had been long plunged was not tinged by it, or only in the fmalleft degree imaginable.

This incruftation alfo adhered much more firmly to the green glafs than to the flint, and when it was fcraped off with the point of a knife, though it left the glafs tranfparent, it was not quite fo well polifhed as before: fo that, probably, the glafs had been, as it were, abraded, the texture being broken, but not fo much as to make it feparate from the tube.

I fhall in this place mention an experiment fimilar to thofe above on the marine acid air itfelf. I buried a flint glafs tube filled with this kind of air in hot fand, and let it continue there fome weeks. When I took it out, it was covered with a white incruftation. I broke the end of the tube under quickſilver, and found that ſeven eights of the whole quantity had been abſorbed, and water imbibed about half the remainder. The very little that was left was phlogiſticated air. This tube had been filled with ſo much care, that I cannot think there had been any common air in it.

I have ſeveral times repeated this experiment, and find that no great degree of heat is requiſite to convert the marine acid air into this white ſubſtance. It is not at all affected by ſpirit of ſalt.

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SECTION XI.

Of the Expoſure of various Subſtances containing Spirit of Salt to a continued Heat.

SOME of the phenomena that attended the expoſure of ſaturated ſolutions of ſpirit of ſalt to a long continued heat were not a little remarkable.

Spirit of ſalt ſaturated with the *ruſt of iron* did not boil ſo ſoon as pure ſpirit of ſalt, in equal tubes, both hermetically ſealed, which muſt be underſtood to be the caſe in all theſe experiments. In four or five hours a white incruſtation was formed on the ſides of the glaſs, after which the liquor was leſs viſcid, and boiled more freely than before.

Letting it ſtand to cool in the night, the next day the tube, having lain in an horizontal poſition, was almoſt covered with ſmall incruſtations, ſlightly adhering to it, having been depoſited from the liquor. The tube being replaced near the fire, all the concretions diſappeared to a conſiderable from the end of the tube to which the heat was applied, thoſe next to the bottom being perfectly white, while the reſt were brown, the colour of the ſaturated acid. The reaſon of theſe various phenomena I do not clearly underſtand.

Spirit of ſalt ſaturated with *red precipitate*, or with *flowers of zinc*, expoſed to heat in the ſame manner, and at the ſame time with the ſolution of the ruſt of iron, underwent no viſible change whatever, and no incruſtation was formed within the glaſs tubes in which they were contained.

Theſe experiments were made before a common fire, and the heat was not applied longer than a few hours two or three different times. The expoſure of the ſame ſubſtances in a ſand furnace for a longer time produced a greater effect.

The red precipitate in ſpirit of ſalt, which had been expoſed to this heat three or four days, was ſtill colourleſs; but from the top of the tube, to about the middle, on the ſide to which it had been inclined, it was covered with beautiful white cryſtals,

tals, confifting of many fine fpiculæ like hairs. There was alfo a fmall white incruftation on the oppofite fide, juft above the furface of the acid.

This experiment was made on the 30th of September. Examining the tube that contained this faturated folution of red precipitate on the 19th of January following, I found that when it was cold, the whole was perfectly folid, and white. With a little heat it became liquid, and tranfparent, as at the firft, but in the cold it was always folid. In this ftate it continued feveral months, when the tube was broke by an accident. The infide of the glafs tube above the concreted folution was covered, on the inclining fide, with white *fpiculæ*, efpecially about the middle, where they formed a folid mafs, but confined to a fmall fpace. Nearer the bottom of the tube, the fpiculæ were longer, but fewer in number. I imagine that the fpirit of falt had diffolved part of the fubftance of the glafs, the watery part entering into the faline fubftance formed by their union, as in the former

former experiments, and that there was not moiſture enough left to keep the ſolution fluid, except when it was warm.

When I kept a tube containing a quantity of this ſaturated ſolution a day or two before a common fire, there was a ſmall quantity of whitiſh matter in the liquor itſelf, but very little adhered to any part of the tube.

A ſaturated ſolution of flowers of zinc expoſed to the ſame degree of heat, in the ſame manner, for three or four days, was tranſparent, but had depoſited a browniſh matter, and there was a ſlight whitiſh incruſtation about four inches above the ſurface of the liquor. The appearance was nearly the ſame when the tube was examined about four months afterwards, when it had been in the ſand furnace all the time.

A ſaturated ſolution of *lime* expoſed to the heat of a common fire in a glaſs tube made a large incruſtation on the glaſs.

Since common ſalt contains the marine acid, I ſaturated a quantity of water with it, and

and expofed a little of it to the heat of the fire, in a long glafs tube hermetically fealed, making it boil about an hour. When it was cold, I perceived that the liquor was fenfibly cloudy. I then placed the tube in the fand furnace, and examining it about a month after, the folution was tranfparent, and the glafs had acquired a thick white incruftation an inch above the furface of the liquor. Four months afterwards, the folution was ftill tranfparent, and the white incruftation extended half an inch below the furface of the liquor. There was alfo a thinner incruftation about two inches long, at the diftance of three inches above the furface, and likewife fpecks of a whitifh matter in feveral parts of the tube to the very top of it. Thefe incruftations were either formed by the marine acid difengaged by the folution, or by the watery part of it corroding the glafs, or perhaps by both thefe caufes.

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SECTION XII.

Experiments relating to the Difcharge of the Colour of various Solutions made by the Marine Acid.

I HAVE mentioned one inftance in which a coloured fpirit of falt had its colour difcharged by a fecond faturation. Afterwards I accidentally found another fubftance that produced the fame effect; and having had the curiofity to carry my obfervations relating to this fubject to fome length, I was fortunate enough to fucceed in the inveftigation beyond what I expected, though much ftill remains to be afcertained with refpect to it.

I had been extracting air from *cream of tartar* by means of oil of vitriol, firft in a phial with a ground ftopper, with very little heat, and then with a red hot fand heat. The black refiduum I diffolved in fpirit of falt, which was of the ufual ftraw colour, and I found that inftead of giving any colour to it (which confidering the blacknefs of the fubftance ſubſtance, I fully expected) made it perfectly colourleſs like water; and, during the ſolution, I perceived a ſtrong ſmell of liver of ſulphur. Afterwards I had the ſame reſult from the reſiduum of a mixture of oil of vitriol and cream of tartar, which had not been calcined. This matter being expoſed to the open air attracted the moiſture of the atmoſphere very ſtrongly, and had the conſiſtence and ſmell of treacle. In time the more ſolid part formed itſelf into a cake, and pouring off the watery part, I dried the reſt for other purpoſes.

After this I had the ſame effect from the mere *coal of cream of tartar*, calcined to blackneſs. The ſmell of this tartar, during the calcination, exactly reſembled that of ſugar or treacle. To ſpirit of ſalt, this coal, which was diſſolved by it very rapidly, gave no colour whatever; but, on the contrary, diſcharged whatever colour it had acquired by any other impregnation; provided that, as in all the former caſes, the colour was not too deep in proportion to the quantity of the coal of tartar. For the

purpoſe

purpoſe of theſe experiments I happened principally to make uſe of a quantity of ſpirit of ſalt which had acquired a beautiful yellow colour from the ſolution of the white matter that remains after diſtilling to dryneſs a quantity of common oil of vitriol, the colour of this ſolution being eaſily diſcharged by a ſmall quantity of the coal of tartar, and thereby anſwering my purpoſe remarkably well in the ſubſequent experiments.

Tartar calcined to whiteneſs (the black colour being expelled by long continued heat) had the ſame effect on the coloured ſpirit of ſalt with the black coal of tartar, and was diſſolved with equal rapidity. The power of this coal of tartar to diſcharge the colour of ſpirit of ſalt was exhauſted by being uſed for this purpoſe. For when it had diſcharged the colour of one impregnation, and was taken out, well waſhed, and dried, it had no effect a ſecond time. It alſo loſt this virtue by being waſhed with ſpirit of ſalt that had not been coloured with any impregnation.

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The ſolution of ſalt of tartar in ſpirit of ſalt very much reſembled the ſolution of the coal of tartar in it, and after the longeſt calcination that I ever gave the coal of tartar, it ſtill yielded a great quantity of fixed air. But, notwithſtanding this reſemblance, the *ſalt of tartar* had no effect on the colour of this acid, neither was the colour ſenſibly affected by an impregnation with fixed air. It was not, therefore, the fixed air in the tartar that had produced this effect.

I have obſerved that the coal of tartar, during its ſolution in the ſpirit of ſalt, emitted a ſmell of liver of ſulphur. This gave me the hint of trying liver of ſulphur itſelf, and I preſently found it anſwered my purpoſes much better than the coal of tartar itſelf, diſcharging inſtantly the deepeſt yellow colour that the acid ever acquired. It was evident, therefore, that the diſcharge of the colour was owing to ſomething common to the coal of tartar and liver of ſulphur, which I imagined to be phlogiſton in ſome common ſtate, an hypotheſis which was rendered more probable by an experiment that will be recited preſently; though it is certainly not favoured by the flowers of zinc producing the ſame effect.

The moſt remarkable circumſtance relating to the diſcharge of the colour of ſpirit of ſalt is that, when it is expoſed to the open air, it never fails to recover the colour that had been diſcharged, and a very little air confined in the ſame phial with it is ſufficient for the purpoſe.

The firſt time that I obſerved this, was when I had coloured a quantity of ſpirit of ſalt with the reſiduum of oil of vitriol, which, as I have obſerved, gives it a yellow tinge, and had diſcharged the colour by the ſolution of black coal of tartar. For when I had, for ſome purpoſe or other, taken out the ſtopper of the phial in which it was kept, I found that, in a few days, it had completely recovered its former yellow colour.

When this proceſs is made in a tall phial, it is pleaſing to obſerve how the reſtoration of the colour begins at the top, and, in the courſe

courſe of a few days, deſcends gradually to the bottom. But let it be kept ever ſo long in a phial cloſe ſtopped, where no air can have acceſs to it, and it will always continue colourleſs. I once kept a quantity of ſpirit of ſalt, firſt coloured, and then rendered tranſparent, in this manner, ſeveral months, in a phial with a glaſs ſtopper, and it continued colourleſs all the time; but upon taking out the ſtopper, it recovered in a few days its original colour, but more coal of tartar diſcharged this colour a ſecond time.

I once had an inſtance of a quantity of this acid recovering its colour ſpontaneouſly in a manner that I cannot well account for. After the colour had been completely diſcharged, it had been confined in a phial with a glaſs ſtopper, and a very ſmall quantity of air. In theſe circumſtances it recovered its colour in two or three days; but, in a few days more, without having been opened in the mean time, it was found colourleſs again. I ſuppoſe there might remain enough of the black coal in the acid

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to diſcharge all the colour it had been able to recover by means of the air on its ſurface; but then why did not the ſame cauſe prevent its recovering its colour at all?

Something ſimilar to this was the following obſervation. On the 19th of November 1778, having a quantity of ſpirit of ſalt which had acquired a deep yellow colour from various impregnations, I took two equal quantities of it, and putting them into equal phials, I diſcharged the colour of one of them with *liver of ſulphur*, and that of the other with *flowers of zinc*, obſerving that a large quantity of the latter was neceſſary for the purpoſe, but only a very ſmall quantity of the former. In the diſcharge of the colour with the flowers of zinc I alſo perceived a ſlight ſmell of liver of ſulphur.

Theſe two phials, containing equally colourleſs ſpirit of ſalt, I covered with equal jars of common air ſtanding in water; and in a day or two perceived that the acid in both of them had begun to recover its yellow colour; but that in which the

colour

colour had been difcharged with flowers of zinc went no farther than about half way towards the bottom of the phial, and then the acid gradually became colourlefs again; whereas the acid in the other phial completely recovered its former colour. Thus they continued without any appearance of a farther change, till December 3, when I examined the air to which they had been expofed, and found it nearly in the fame ftate in them both, and confiderably worfe than common air. With the air expofed to the phial with the flowers of zinc the meafures of the teft were 1.35, and with the liver of fulphur, 1.33. With the common air, at the fame time, they were 1.2. Confidering the difference of the circumftances in this experiment, I had expected a greater difference in the refult.

SECTION XIII.

Of the Vitriolic Acid.

THE moſt remarkable obſervation that I have made relating to the vitriolic acid will be found under the article of *nitrous vapour* with which I impregnated it, and which precipitates all the vitriolic acid, in the form of cryſtals, and leaves the water in the poſſeſſion of the nitrous acid. My other obſervations are neither numerous, nor important.

It is well known that there is an earthy matter in the common oil of vitriol. But this I find is not eſſential to it: for almoſt the whole of it is depoſited in the firſt diſtillation, and when I diſtilled this acid a ſecond time there was little or none of it left. On this account, oil of vitriol that is concentrated by merely boiling the water out of it is not quite tranſparent, the earthy matter being diſperſed in it, but that which has been diſtilled twice may be as highly con-

concentrated as poſſible, and yet be as tranſparent as water itſelf.

I tried the effect of a long continued heat on this as well as on the other acids, but was not able to make any ſenſible change in it. Whatever vapour was raiſed from it was condenſed again; though, in time, the glaſs veſſel in which it was contained was a little corroded.

When a quantity of oil of vitriol is thrown into an open fire, it is evaporated in denſe white fumes. Theſe I had ſuſpected to be the acid vapour joined to the water which it found in the atmoſphere; but I find the ſame white denſe vapours in the cloſeſt veſſels, and when the acid is in its moſt concentrated ſtate; ſo that it muſt be the natural form of the vapour in a certain degree of heat. For in a greater degree of heat the ſame vapours are colourleſs. It cannot, however, be ſaid that the acid of vitriol, in a ſtate of the greateſt concentration to which we can bring it, is wholly uncombined with water. The whiteneſs of theſe vapours is probably owing to the inequality of denſity between them and the air, or vapour of other kinds, with which they are mixed when it is beginning to condenſe. For when they are ſo copious as to exclude every thing elſe, and the heat is ſo great as to prevent condenſation, this vapour is as pellucid as the glaſs itſelf.

I expoſed to the heat of a common fire, in a glaſs tube about half an inch in diameter, and four feet long, as much oil of vitriol as filled about two inches of it; and making it boil violently for two hours, obſerved no change of colour in it. At the firſt boiling a quantity of opake vapour iſſued out of it, and kept dancing there three or four inches above the ſurface of the acid all the time it continued to boil; but when the whole was cold it was as tranſparent as ever. I made it boil about an hour after this, but the quantity of white vapour was not ſenſibly increaſed, as is the caſe with the red vapour of the ſpirit of nitre. When, after this, I ſoftened the glaſs of the tube with a blow-pipe, it was preſſed inwards, but not ſo much as to give me a ſuſpicion that

that there had been any diminution of the air within the tube.

In the courſe of the diſtillation of vitriolic acid theſe white vapours never fail to be raiſed by heat, to retire to a diſtance from the acid during the increaſe of the heat, and to return towards it during its decreaſe, exactly like the red vapours with reſpect to the nitrous acid, which is a phenomenon altogether independent of water.

When I melted with a blow-pipe a part of a hot glaſs tube containing vitriolic acid, which was quite tranſparent, there ruſhed out of it, with vaſt impetuoſity, a denſe cloud of white vapour. Within the tube, where the vapour was equally diſtributed, there was no perceivable opacity; but on ruſhing out it began to be diſperſed, I ſuppoſe, unequally, as well as to unite with the watery vapours which it met with.

I then tried the effect of a ſtill ſtronger heat, putting a quantity of the concentrated acid of vitriol into a glaſs tube, which I placed in a ſand furnace, in which it continued three or four hours, the ſand being red hot. The next day, when it was cold, I found a white incruſtation quite round the glaſs, a little above the ſurface of the acid, and likewiſe another incruſtation about an inch higher up in the tube, a little of which was waſhed off by ſhaking the acid. Still the glaſs, when ſoftened by heat, was preſſed inwards, ſo that there was no permanent elaſtic vapour formed.

After this I expoſed a larger quantity of the vitriolic acid to a more moderate ſand heat, for a greater length of time. But after being expoſed ſeveral months in a ſand furnace, there was no ſenſible change made in the acid, nor did any material obſervation occur to me. A little whitiſh matter, indeed, was obſerved at the bottom of the tube, but it ſeemed to be nothing more than the effect of the corroſion of the glaſs. For when it was taken off, the ſurface of the glaſs was found to have loſt its poliſh.

Whence comes that white matter that is depoſited in the concentration of oil of vitriol I cannot tell, but it is probably ſome

earthy

earthy matter derived from the fulphur from which it is extracted.

Perhaps fome of the following obfervations may ferve to throw a little light upon it. Having got a fmall quantity of it by the concentration of a large quantity of oil of vitriol, I poured fpirit of falt upon it, and obferved that it was not prefently affected by it, but in time it was completely diffolved, and gave a beautiful yellow tinge to that acid, the fubftance itfelf alfo turning yellow. In the nitrous acid it was not affected at all, and retained its whitenefs.

. Having wafhed a quantity of this white fediment in diftilled water, I evaporated that water in an open glafs veffel, and obferved that, towards the end of the procefs, a fmall concretion was formed and left upon the glafs, as if this matter had been in part diffolved in the water, and when it was nearly ended, a denfe white vapour iffued from it, which had the fmell of treacle or burnt fugar.

I formerly obferved that a quantity of black matter was formed by heating ether

in

in oil of vitriol, in order to get from it vitriolic acid air. This matter was but little affected by ſpirit of ſalt, but it gave a yellow tinge to it. The quantity of this matter does not ſeem to depend upon the quantity of the ether in the mixture. For when I heated equal meaſures of ether and oil of vitriol, I did not get more of it than when I had uſed a much leſs proportion of the ether. This ſubject deſerves to be inveſtigated farther.

SECTION XIV.

Of the volatile Vitriolic Acid, and Vitriolic Acid Air.

THE volatile vitriolic acid, though produced from the fixed vitriolic acid, is very conſiderably different from it, eſpecially as it may be diſlodged from its baſis by the vitriolic acid, juſt as other weaker acids are diſlodged by thoſe that are thence called the ſtronger. But that volatile vitriolic acid is capable, however, of being brought

brought back to the ſtate of the common vitriolic acid, and becoming the ſame thing that it originally was, ſeveral experiments ſhew. At the time of my laſt publication I had found that it was capable of diſſolving iron and zinc, and of producing inflammable air, which is the property of oil of vitriol: but I had a more deciſive proof of the ſame thing when, to water ſaturated with vitriolic acid air, I had, for another purpoſe, put ſome earth of alum till it was ſaturated. For, after ſix months, in which this ſolution had been expoſed in an open phial, and one third of it was evaporated, I obſerved many tranſparent cryſtals formed at the bottom of the phial, as well as an incruſtation on the ſides of the phial above the ſurface of the liquor. Theſe cryſtals were all triangular, of a conſiderable thickneſs, connected with each other, and when examined appeared to be *alum*, which is known to be the ſaline ſubſtance formed by the ſame earth, and the proper vitriolic acid. But the following experiments in which it will appear that real ſulphur is formed by means of the volatile vitriolic acid, exhibit a much more remarkable fact, and is another proof of the mutual convertibility of theſe acids into one another.

Having expoſed various liquid ſubſtances to a continued heat in a ſand furnace, among others I placed in it a glaſs tube, about an inch in diameter at the bottom, tapering to a point at the top, about two feet and an half long, cloſed hermetically; when I had put into it about an ounce meaſure of diſtilled water ſtrongly impregnated with vitriolic acid air, with nothing more than a random expectation of ſome change or other taking place in it. This was on the 9th of September 1777, but the reſult was much more curious than I could poſſibly have imagined *a priori*. I ſhall note the appearances as I obſerved them, at the ſeveral intervals in which I examined this tube.

On the 30th of the ſame month this impregnated water, which continued tranſparent to the end of the proceſs, had depoſited a ſmall quantity of black powder; and

and alſo a bit of matter exactly like *ſul-phur* about one eighth of an inch in diameter lay among it. Small pieces of the ſame matter floated on the ſurface of the liquor, and ſtreaks of the ſame coated part of the inſide of the tube an inch above the liquor. From the top of the tube to within about eight inches above the liquor, were beautiful white cryſtallizations, like *ſpiculæ,* diſpoſed irregularly, but generally in the form of ſtars, the glaſs being perfectly tranſparent between them.

In this ſtate the tube continued, the cryſtallizations increaſing, and ſeveral times changing their places, to the 20th of January following, when an end was put to the proceſs. Excepting, however, a place of a few ſquare inches near the ſurface of the liquor, all the lower half of the tube was quite free from them, but from thence to the top it was pretty thick and equally covered, exhibiting a moſt pleaſing appearance.

In order to obſerve the *time,* and the *manner* of the formation of theſe cryſtals, in

a greater

a greater variety of circumſtances, I placed in the ſand furnace at different times, a ſtrong glaſs tube about nine inches long, and a quarter of an inch in diameter, which I ſunk pretty deep in the ſand, in order to give it a greater degree of heat; and alſo two tubes about four feet long, one of them half an inch, and the other a quarter of an inch in diameter, putting into the ſhort tube a quantity of the impregnated water about an inch in length, and into the long tubes two inches and a half.

The ſhort tube had been put into the ſand on the 11th of Auguſt, and on the 30th of September following the liquor was tranſparent, but the top and part of the middle of the tube had many white ſtars like cryſtallizations.

Of the long tubes the ſmaller had begun to have cryſtallizations, about one third from the bottom in about a fortnight, and the wider in about a month. When they were examined on the 19th of January 1778, the large tube had more cryſtallizations than the ſmaller, the greateſt quantity of them about five inches above the ſurface of the liquor, but they were all on one ſide of the tube, and there were others about ſix inches above theſe. There were alſo very many between the ſurface of the liquor and two inches above it. The ſmaller tube had no cryſtals near the ſurface of the liquor, but a good many about five inches above it, and the greateſt quantity was about eighteen inches above it. Neither of theſe tubes had any cryſtals in two thirds of the upper part of them.

Applying the flame of a candle with a blow-pipe to the ſmaller of the long tubes above-mentioned, the glaſs was preſſed violently inwards; ſo that it was evident there was a decreaſe of elaſtic matter within the tube, which therefore probably entered into the cryſtals. If any part of the liquid touched the hot glaſs, a denſe white fume was excited, exactly like that from the oil of vitriol. Taking off one half of the tube, and then opening it under water, it was half filled with water, and the air within in it was completely phlogiſticated, which

which agrees with my former obſervations, of the vitriolic acid air imparting phlogiſton to common air.

When I heated the dry cryſtals, the ſame white cloud was raiſed, and the cryſtals were by this means diſperſed into a kind of duſt, that incruſts the glaſs. For I applied the heat on the outſide of the tube.

The liquor itſelf was ſtill extremely acid, and the ſmell of it very pungent; ſo that, probably, only a ſmall part of the vitriolic acid air with which it was impregnated had entered into theſe cryſtals, numerous as they were.

The cryſtals were eaſily ſhaken off from the ſide of the tube, when it was waſhed with the liquor, and they continued undiſſolved in it.

The preceding obſervations were made preſently after the tubes in which the cryſtals were formed were taken from the ſand furnace; and in this ſtate they continued near a year, in the courſe of which I had ſhewed them to ſeveral of my chemical friends, who expreſſed much ſurpriſe at

 the

the ſight of them. At length I opened the tube that contained the greateſt quantity of theſe cryſtals, firſt obſerving that, when I ſoftened the glaſs, it was ſtill preſſed inwards.

The cryſtals, I found, were not diſſolved in ſpirit of ſalt, and when they had been waſhed, and dried, they had the colour, and ſmell of *ſulphur*; and being laid on a hot iron burned with a blue flame, ſo as to leave no doubt of the identity of the ſubſtances.

To form this ſulphur, I conjecture, that the phlogiſton which had rendered the acid volatile, in this expanded and confined ſtate, had been compelled to form that very different and peculiar union requiſite to make ſulphur. The fact is certainly a remarkable one.

Having obſerved theſe curious effects of the impregnation of water with vitriolic acid air, I expoſed to the ſame heat, in ſimilar circumſtances, ſpirit of wine, and oil of turpentine, ſaturated with the ſame kind of air.

The impregnated ſpirit of wine, after being expoſed to this heat about a fortnight was tranſparent, but had many ſlender cryſtals in it, and the greater part of the tube had a thick and whitiſh incruſtation, beginning about three inches above the ſurface of the liquor, and extending about twelve inches, but was thickeſt in the middle.

A ſhort tube, containing a quantity of the ſame impregnated ſpirit of wine, had no incruſtation, but many more cryſtals, in the form of ſpiculæ which ſettled to the bottom of the liquor. Another tube of the ſame length had ſimilar ſpiculæ, and near the top a conſiderable incruſtation not ſpiculine.

The oil of turpentine impregnated with vitriolic acid air, and expoſed to the heat in this manner, from being of a light amber colour, became of a deep brown. The tube in which it was contained was only eighteen inches long, and the upper half of it was covered on one ſide with white incruſtations not ſpiculine.

Whale

Whale oil impregnated with this air, from being brown, had probably become almoſt black. For the tube was broke, but had a very black incruſtation towards the bottom, eſpecially near the ſurface of the liquor.

I alſo expoſed to the ſame heat tubes containing vitriolic acid air only, having firſt filled them with quickſilver, then with this kind of air, and afterwards ſealing them hermetically with a blow-pipe; and the reſult was ſimilar to thoſe in which the impregnations were concerned.

One tube of this kind that had been buried in the hot ſand on the 11th of Auguſt, being examined on the 30th of September, was found in the following ſtate. The upper part of the tube was half covered with white cryſtals, but much ſmaller than thoſe in the tubes containing the water impregnated with this air.

Another tube containing the ſame kind of air, which had been buried in the ſand a longer time, was found quite covered with white cryſtals, and a ſmall part of the tube was black, probably from ſome external accidental cauſe. The end of this tube being broke under quickſilver, it filled one third of it, and water abſorbed all that remained of the air, except a very ſmall bubble. This water had the ſmell of water impregnated with vitriolic acid air.

It is evident that this acid air had been in part thrown into the form of ſolid cryſtals by this expoſure to heat; but then with what ſubſtance was it united, or did the air contain within itſelf the principle of this combination, but wanted the action of ſuch an external force to bring them into this kind of union?

I have ſeveral times repeated this experiment, and have never failed to find the inſide of the tubes that had been filled with vitriolic acid air coated with this white matter; but it is ſo exceedingly ſlight, that I cannot make many obſervations upon it. I am rather ſurprized to find that it does not ſeem to be *ſulphur*, which is formed from the heating of water impregnated with the ſame kind of air. For ſpirit of ſalt falt feems to diffolve it all. At leaft the tube is wafhed perfectly clean with it, and I could not difcern any thing in that acid afterwards. But this may be owing to the very fmall quantity of it, though it be fpread on fo great a furface, and to the extreme minutenefs of the particles of which it confifts.

SECTION XV.

Of the Phofphoric Acid.

HAVING made fo many experiments on the acids, with a view to reducing them to the form of air, and upon their properties when exhibited in that new form, it might have been expected that I fhould, before this time, have taken notice of the *phofphoric acid*, which is fo remarkably different from the other acids, and which bears fo near a relation to the animal oeconomy. The true reafon of this feeming neglect of fo important a fubject of experiment was the expence neceffary to procure it in any tolerable quantity. At length, however, I procured a quantity ſufficient for a few experiments, not undeſerving of being related.

Chemiſts do not need to be informed of the method of procuring this liquid acid from ſolid phoſphorus; but for the ſake of perſons of only a general philoſophical turn, like myſelf, it may be worth while to obſerve, that this acid is eaſily procured, with time, by expoſing it to the open air in the mouth of a funnel, going into a phial which receives the acid, as the phoſphorus gradually waſtes by this kind of accenſion. It muſt be ſet in a place neither very cold, nor very warm. But this depends upon the conſiſtence of the phoſphorus, and other circumſtances, which muſt be learned by experience. If it ſmokes very much, it is a ſign that it is too warm, and is in danger of taking fire, in which caſe it may be ſaved by plunging it inſtantly in water.

Having procured my phoſphorus, I firſt obſerved, that the water in which it had been long kept had nothing acid in it.

For

For, being mixed with water made blue with the juice of turnfole, it did not affect its colour, which fhews that no proper decompofition of it takes place in water. Having then expofed it to the open air, in the manner defcribed above, I got a quantity of the acid with which I made the following obfervations.

With refpect to *air*, this acid very much refembles radical vinegar, or rather the vitriolic acid. For though the application of heat converts it into vapour, it is all condenfed again in the temperature of the atmofphere, and no part of it remains permanent elaftic air. I made the experiment in a glafs tube bent a little like a retort, the open end of which turned up into a veffel filled with quickfilver, and immerfed in a bafon of the fame. When I made the acid boil, the vapour paffed into the recipient, but it was wholly condenfed there, and the liquor fo collected did not differ, as far as I could perceive, from what it had been before the evaporation.

As, like the vitriolic acid, this gave no air of itſelf, I thought that, like this acid, it might poſſibly give ſomething ſimilar to the vitriolic acid air by means of ſubſtances containing phlogiſton. With this view I kept it in a boiling heat both with quick-ſilver, and alſo with ſpirit of wine, but without any effect; and even the common air, that was expelled from the phial in which the experiment was made, was not ſenſibly phlogiſticated.

This acid, however, reſembled that of vitriol and radical vinegar in this, that it readily diſſolved iron, eſpecially with the aid of a little heat, and with it yielded a ſtrong inflammable air. But there is ſomething more remarkable in the produce of inflammable air from it by means of minium.

In order to try whether this acid had any of the properties of the nitrous, I mixed it with ſome minium out of which all the air had been expelled by heat. This ſubſtance, in this ſtate, I had found, when mixed with nitrous acid, yields dephlogiſticated air, but no air at all with the vitriolic or the marine acid. The phofphoric acid mixed with this minium with little or no fenfible heat, but the mixture expofed to the flame of a candle yielded air very plentifully, and it was very turbid. I received it in lime water, but it did not precipitate the lime, except in the fmalleft degree. The air I got in this method was not affected by nitrous air, nor did it affect common air, but was ftrongly inflammable, burning with a bright white flame; and the fmell of the air was the fame with that of the ftrong fmell of phofphorus. The yellow minium became of a darkifh grey colour, or nearly black by this procefs.

Having a quantity of the mixture of phofphoric acid and fpirit of wine, remaining from the experiment above-mentioned, and not being willing to lofe it, I likewife mixed it with fome of the fame minium, and I had the fame refult. The common air that was firft expelled from the furface of the veffel in which the experiment was made was not much injured, the next that came had a fmall quantity of fixed air in

it;

it; but all the remainder was ſtrongly inflammable, burning with a yellow flame, the next was more weakly inflammable, and the laſt produce was phlogiſticated air only.

It ſhould ſeem that the earth of the minium enters into the compoſition of this inflammable air, ſince the acid itſelf could not ſupply it, and the air was exceedingly turbid when it was firſt produced. But I ſhould think that the phlogiſton muſt have been ſupplied from the acid, ſince the minium, I believe, does not contain it.

About the time that I was making theſe experiments I was making obſervations on the expoſure of a variety of fluid ſubſtances to a long continued heat. I therefore treated this acid in the ſame manner, firſt in a long glaſs tube, held in an inclined or nearly perpendicular poſition, and then in a horizontal one, expelling the acid by the heat from one part of the tube to the other; the reſult of which proceſs was remarkably different from that of the other.

In

In a glaſs tube about thirty inches in length, and one third of an inch in diameter, I put as much of this acid as filled about an inch of the tube in length, and making it boil, there was a white vapour at the height of about fifteen or eighteen inches above the ſurface of the acid, continually dancing up and down as it boiled. At and below this part of the tube, it was very hot, but immediately above it was quite cold. I kept the acid boiling ſeveral hours without any ſenſible change.

Though the phoſphoric acid was not changed by boiling ſeveral hours in the courſe of two days, in a glaſs tube hermetically ſealed, and placed in nearly a vertical poſition, yet when I applied the flame of a candle to any part of the tube, after the acid had left it moiſt (when it had been made to flow to the other end) the glaſs was inſtantly covered with a white incruſtation; and repeating this proceſs, at each end of the tube alternately, I quickly made the whole ſolid. At leaſt there was no more moiſture in the tube than adhered to the ſides of it,

and

and could not be made to flow at all. This experiment I repeated in ſeveral tubes, and always with the ſame event, whatever was the quantity of the acid.

When the tube was made very hot there would ſometimes be flaſhes of light in the inſide, extending the whole length of the tube; and of theſe there were ſometimes three in the ſame tube at different times. Whenever this happened, a part of the tube always acquired a thin coating of orange coloured matter, ſuch as remains upon glaſs when phoſphorus is really ignited upon it in the open air.

The white matter thus left in the glaſs tubes attracted no moiſture from the atmoſphere, at leaſt no ſenſible quantity of it, and it was not at all affected by ſpirit of ſalt. It did not even long retain any ſenſible acidity; for when it had been waſhed ſeveral times, the water in which it lay did not even turn the juice of turnſole red.

If I be aſked what I think becomes of the moiſture which rendered the phoſphoric acid liquid in this proceſs, I ſhould ſay that

that, as in the ſimilar experiments with the marine acid, it diſſolves the glaſs, and with it the acid and water both unite in a ſolid form, as in other cryſtalizations; and ſince I made theſe experiments, I have been informed by Dr. Ingenhouſz, a man of a truly philoſophical and experimental turn, that the phoſphoric acid, when hot, diſſolves glaſs, exactly like the fluor acid.

SECTION XVI.

Obſervations relating to the black Powder produced by the Agitation of impure Quickſilver.

BOERHAAVE found that quickſilver, by very long continued agitation, was in part converted into a black powder, which is often ſeen on the ſurface of it, and which, I believe, is generally deemed to be a partial calx of this metal, the mercury having parted with ſome portion of its phlogiſton in this proceſs. It is thought, however,

however, that it is no great proportion of its phlogifton that it parts with in order to affume this new form of black powder, becaufe it is not poffible to expofe it to any confiderable degree of heat without completely revivifying the whole of it. Even mere trituration has been obferved to have the fame effect. On this account, fome do not confider this procefs as a proper calcination, but fuppofe the mercury only to have affumed a *new form*, really containing all the phlogifton it was ever poffeffed of. And it will be in my power to fhew, in the courfe of this work, that there are feveral cafes in which mere heat produces the fame effect, to appearance, with the addition of phlogifton.

Notwithftanding this, I think it will appear from the refult of my obfervations on the fubject, that this black powder is really mercury *fuperphlogifticated*, having acquired more phlogifton, inftead of having parted with any that had properly belonged to it; that various fubftances agitated together with mercury give it this over-

charge

charge of phlogiſton, and to appearance reſume it again. I alſo hope to ſhew in one view all the ſteps in the complete progreſs of mercury from this ſuper-phlogiſticated ſtate to its proper dephlogiſticated ſtate, the *precipitate per ſe*, in which it aſſumes four very different appearances. For the greater ſatisfaction of my readers, I ſhall, as I generally have done, relate my obſervations hiſtorically.

Having been under a neceſſity of making much uſe of quickſilver in my experiments relating to air, in order to ſeparate and preſerve thoſe kinds that would have been abſorbed by water, and being frequently obliged to remove my apparatus from the country to London, and from London to the country, I could not help being ſtruck with a quantity of black powder, which I ſometimes found upon the ſurface of my quickſilver; when, at other times, and, as far as I could judge, in the ſame circumſtances, I found very little, or none at all. It was evident, however, that whatever was the *cauſe* of this appearance, the *agitation* of the carriage had contributed to it; for, except in thoſe circumſtances, I never found any of it. At one time I found, after removing my quickſilver, which was about twelve pounds, from London into the country, there was near a pound of this black powder on the ſurface of it. This I thought a great acquiſition, as it was a quantity ſufficient for a variety of experiments.

The firſt thing that occurred to me to do with it was to endeavour to expel air from it by means of heat. Accordingly, I put a quantity of it into a glaſs phial with a ground ſtopper and tube, and, with the heat of a candle, I preſently expelled from it a quantity of air; which being admitted to lime water made it very turbid, and was, in a great meaſure, abſorbed; a proof that the air it had contained was in part fixed air, and the remainder was not ſo much diminiſhed by nitrous air as common air would have been; ſo that no pure air came from this black powder, and conſequently it differed eſſentially from the *precipitate*

per

per se, which would have yielded no fixed air, but the pureſt dephlogiſticated air only. I obſerved alſo, at the ſame time, that the powder at the bottom of the phial, which had been expoſed to the greateſt degree of heat, had become yellow. This was evidently ſomething elſe than could have come from the quick-ſilver, but I did not at that time diſcover what it was. A good deal of the quick-ſilver was revivified by the proceſs.

Expoſing another part of this black powder to a red ſand heat in a glaſs veſſel, I produced a greater quantity of fixed air. Alſo, part of the black powder became yellow, as before; and triturating the whole of it in the palm of my hand, it aſſumed a kind of dirty green colour, and about one half of it was pretty readily converted into quickſilver. Putting the remainder of this greeniſh powder into a thin glaſs veſſel, and holding it over the flame of a candle, about one half of it became a perfectly yellow powder, and the reſt was evaporated, and being in part collected, appeared to be pure quickſilver.

By this means I effected a complete ſeparation of the quickſilver which had conſtituted the blackneſs of the powder, and had a perfectly diſtinct yellow ſubſtance behind, the nature of which an experienced chemiſt would have immediately diſtinguiſhed; and I diſcovered it ſoon afterwards. I preſently concluded that, notwithſtanding this yellow ſubſtance ſeemed to be produced from the quickſilver, and had great ſpecific gravity, it was not of the nature of *precipitate per ſe*, becauſe it had yielded fixed air. With another part of the black powder I found that the fixed air it yielded was ſeveral times the bulk of the powder, but I did not aſcertain with exactneſs what the proportion was.

Being ſtill ignorant of the conſtitution of this black powder, and being, conſequently, unable with certainty to procure a quantity of it, I conſidered what other ſubſtances into which mercury entered had the ſame appearance, and among others I ſuſ-

ſuſpected that *Æthiops Mineral,* which is a compoſition of mercury and ſulphur, might perhaps be the ſame thing, and if ſo, it might be eaſily procured in any quantity for the purpoſe of future experiments. But I preſently found that this ſubſtance, treated in the ſame manner in which I had treated my black mercurial powder, yielded no air all.

Diſappointed in this expectation, and being very deſirous of procuring a quantity of this black powder, I took ſeveral quantities of this quickſilver, in the ſame ſtate in which I had generally uſed it, and therefore, as I hoped, in the ſame ſtate in which it had yielded the black powder before; and in order to treat it as nearly as poſſible in the ſame manner, I put it into ſuch earthen pots as I had before made uſe of in conveying it from one place to another; and farther to promote a more minute diviſion of its parts, I ſometimes put ſand, and other ſubſtances on which I knew it could have no chemical action, into the pot along with it. I then put theſe pots into ſmall boxes, and procured them to be faſtened to poſt chaiſes, and other carriages, and had them brought to me again after they had undergone, at leaſt, as much agitation as the former quickſilver had done in its paſſage from London to Wiltſhire. But this produced no ſenſible effect; the quickſilver, as it appeared afterwards, being then too pure for that purpoſe.

At length it occurred to me that the quickſilver having been uſed for a great variety of purpoſes, and conſequently having been expoſed to a great variety of impregnations, it might have got ſome metallic ones, and particularly from lead or tin. I therefore diſſolved a ſmall quantity of lead in ſome mercury, and preſently found that a very ſlight agitation covered it with black powder, and obſcured all the inſide of the veſſel.

Being now in poſſeſſion of what had been ſo long the object of my wiſhes, and being able to procure this black powder at pleaſure,

ſure, I was preſently led by it to other obſervations both curious and uſeful.

In order to obſerve the nature and progreſs of this operation to more advantage, I filled a glaſs phial, of about ten ounces, one fourth part full of this mixture of mercury and lead; and inverting it in a baſon of the ſame, I agitated it with my hand, and preſently found that the air within the phial was ſenſibly diminiſhed, an evident proof that it was phlogiſticated; and in about ten minutes the diminution amounted to one fifth of the whole, after which no agitation had any more effect upon it. Examining this air, I found, as I expected, that it extinguiſhed a candle. Indeed it was completely phlogiſticated; not being at all affected by nitrous air.

I was now fully ſatisfied that this was what I have called a proper *phlogiſtic proceſs* with reſpect to air, ſimilar to the calcination of metals by heat; the air being affected in the ſame manner, and that when mercury and lead were thus reduced to an amalgam, the ſimple expoſure to air was ſufficient to produce the calcination of one of them at leaſt; and, as I then thought, of both, agreeably to the common opinion concerning the nature of the black powder of mercury.

I was abundantly confirmed in my ſuppoſition, by finding that when, inſtead of common air, I agitated this amalgam in fixed air, nitrous air, inflammable air, or in any kind of phlogiſticated air, no black powder was produced, and thoſe kinds of air remained unaltered. When, indeed, I agitated this amalgam in nitrous air, the ſurface of it preſently aſſumed a blackiſh hue, but this ſoon nearly diſappeared, and no farther agitation produced any ſenſible effect. But when, on the contrary, I made this agitation in dephlogiſticated air, the black powder was generated exceedingly faſt, and the air went on diminiſhing, till what remained was one fourth leſs than the whole.

It now occurred to me that, by means of this agitation, I might expell the whole of any quantity of lead, or other metals, from

from the mercury with which they might be mixed; and I ſoon found it to be an eaſy and excellent method, not at all inferior to diſtillation. As I have repeated this proceſs many times, and always have recourſe to it when my mercury has acquired any metallic mixture, I ſhall deſcribe the manner in which I find it is moſt expeditiouſly done; though a novice in the proceſs muſt not expect to ſucceed perfectly well at the firſt trial.

I take a glaſs phial with a ground ſtopper (ſuch being generally pretty ſtrong) containing ten or twelve ounces of water, and fill about one fourth of it with the foul quickſilver; then, putting in the ſtopper, I hold it inverted with both my hands, and ſhake it violently, generally ſtriking the hand that ſupports it againſt my thigh. When I have given it twenty or thirty ſtrokes in this manner, I take out the ſtopper, and blow into the phial with a pair of bellows, which I do in order to change the air that has become in part phlogiſticated, and knowing that the purer the air is the faſter the proceſs advances.

After a ſhort time, if the mercury be very foul, the ſurface will not only become black, but a great quantity of the upper part of it will be, as it were, coagulated, ſo as to be eaſily ſeparated from the reſt. I therefore invert the phial, and covering the mouth of it with my finger, let out all the mercury that will flow eaſily, and put the black coagulated part into a cup by itſelf. This I preſs repeatedly with the end of my finger, till I make a complete ſeparation of the running mercury from the black powder; and putting the powder by itſelf, I pour back the mercury to the reſt of the maſs out of which it was taken, in order to be agitated with it again.

This proceſs I repeat till I find that no more black matter can be ſeparated; and it is not a little remarkable, that the operator will be at no loſs to know when the proceſs is completed. For the ſame quantity of lead

lead feems to come out of it in equal times of agitation, and confequently the whole becomes pure at once. Alfo, whereas, while the lead was in the mercury, it felt, as I may fay, like foft clay, the moment the lead is feparated from it, it begins to rattle as it is fhaken, fo that any perfon in the room may perceive when it has been agitated enough. *

That the mercury is made quite pure by this procefs I afcertained by diftillation. For having diftilled in a glafs veffel a large quantity of quickfilver, in which both lead and tin had been purpofely diffolved, and which had only been agitated in this manner afterwards, I found nothing more than a light whitifh ftain on the bottom of the retort.

When

* Pure mercury may alfo be diftinguifhed from that which is very impure by this circumftance, *viz.* that a mixture of lead or tin, at leaft, very much diminifhes its attraction of cohefion. For, when pure mercury is contained in a glafs or earthen veffel, there will be a hollow fpace between the metal and the veffel; whereas if there be lead or tin in it, the whole furface, even to the place of contact with the veffel, will be perfectly level.

When a quantity of the black powder is procured, it is very eaſy, by diſtillation, to ſeparate the mercury from the calx, and I do not know a readier method of procuring the calx of lead, or tin, and perhaps the calx of other metals alſo. The quantity of black mercurial powder is very conſiderable in proportion to the lead or tin mixed with it; though it is not eaſy to aſcertain this with exactneſs, becauſe, in endeavouring to ſeparate the powder from the running mercury a good deal of it is, by mere trituration, converted into running mercury; and I do not know but that, in time, the whole might be reſtored by this means, and the calx of lead, &c. be got quite pure. However, from the following experiments it will be ſeen what proportion they generally bear to each other, after a tolerably careful ſeparation. It will be ſeen alſo, that when all the quickſilver that was converted into black powder is expelled from lead or tin by heat, there will remain more weight of the calx than there was of the metal; as might

might be expected. But as I applied more heat than was neceſſary to ſeparate the quickſilver, a good deal of the air, and whatever elſe contributes to the additional weight of the calx, is, no doubt, expelled with it.

Having mixed 1 dwt. of lead with about five pounds of quickſilver, I expelled it all by agitation, in the method deſcribed above; when, weighing the black powder, it was found to be 1 oz. 10 dw. 5 gr. ſome particles of the running mercury being, however, ſtill viſible in it. When the quickſilver was expelled by heat, the calx of the lead appeared in the form of a browniſh powder, and weighed 1 dw. 5 gr.

Having mixed 1 dwt. of tin with the above-mentioned quantity of quickſilver, and having expelled it again by agitation, the black powder, with ſome ſmall globules of quickſilver mixed with it, weighed 2 oz. 1 dw. 5 gr. and the calx, which was a tolerably white powder, weighed 1 dw. 7 gr.

The

The ſeparation of tin from quickſilver by agitation is not effected near ſo ſoon as lead. It requires at leaſt four times the labour. It alſo requires proportionably more time to ſeparate the black powder from the thick amalgam, in the manner deſcribed above,

Quickſilver is ſeparated from lead or tin when the maſs is agitated in *water*, as well as in air, but it ſeems to require more time. In this proceſs it is alſo eaſily perceived when all the baſe metal is expelled; the phenomena of the agitation of this amalgam and of pure mercury in water being very remarkably different. It is even eaſy to perceive, by this means, in a moment, whether the quickſilver be pure or not. For if it be impure, the water becomes opake the moment the agitation commences, which is by no means the caſe with pure quickſilver, eſpecially if the water in which it is agitated has not been uſed for this purpoſe before. Alſo, the black matter ſuſpended in the water in which

which pure quickſilver has been agitated is (except in a caſe that will be deſcribed hereafter) preſently depoſited; whereas the water in which the amalgam has been agitated does not become clear in ſeveral days. It may alſo be perceived how the quickſilver approaches towards purity, by this depoſit being made more or leſs readily.

Alſo, the phenomena during the agitation in theſe two caſes are ſtrikingly different, though not eaſily deſcribed in words. More eſpecially, the mixture of quickſilver with lead or tin does not ſeem to admit the water to mix with it, whereas pure quickſilver, by violent agitation, may be ſo thoroughly mixed with the water, that it will ſometimes be ſeveral ſeconds after the agitation is diſcontinued, before it have entirely diſengaged itſelf from the water; and in doing this it exhibits a very pleaſing ſpectacle. By this means, as in the proceſs without water, it may be perceived at once when the ſeparation of the baſe metal, and the mercury is completely effected.

Having

Having a large quantity of water made very black with the agitation of a mixture of quickſilver and lead, I agitated a quantity of common air in it a long time, and let it ſtand ſeveral days; but the air was not ſenſibly injured by this means; ſo that though this water and the calcined amalgam ſuſpended in it do contain phlogiſton, it is not by this means imparted to the air.

I evaporated a pint of the diſtilled water in which quickſilver and tin had been agitated, and which had ſtood till it was quite tranſparent, when a white ſediment remained, but it did not weigh more than a few grains.

SECTION XVII.

Of the Agitation of pure mercury in water.

AGITATION in pure water will convert the pureſt quickſilver into black powder, and much more ſpeedily than it can be effected in air; but when this is produced in water, this ſtate of the quickſilver is not permanent. But it will give my reader more ſatisfaction, if I deſcribe the phenomena of this proceſs juſt as they occurred to me.

I agitated a pound of pure quickſilver a few minutes in diſtilled water, when I obſerved that the water had become opake, with particles of a black matter, ſo as to be impervious to the light. This proceſs I repeated ſeveral hours, changing the water as it became black.

When any quantity of water had been once uſed for this purpoſe, the ſame effect was produced much ſooner than it was with freſh water; ſo that, though the freſh water and this could not be diſtinguiſhed by the eye, it was preſently perceived which water had been uſed before.

After I had continued this proceſs, which was in a ten ounce phial, with a ground glaſs ſtopper, about four or five hours, though with ſome interruption, I found that the quickſilver had loſt 2 dwts. of its weight. But, agitating it again little more than an hour, with the ſame water that I had uſed before, I found it had loſt in all 5 dwts.

This proceſs went on the beſt when I uſed three or four times the bulk of water with the quickſilver.

That the *air* contained in the phial together with the water had nothing to do in this buſineſs was evident, becauſe the very ſame effect was produced when the phial was filled up with water only, ſo as to exclude all the air; and this is the manner in which I generally make this experiment.

This black matter diffuſed through the water becomes white running mercury when

when it is expoſed to the open air only. No trituration, or operation of any kind, is requiſite for this purpoſe.

The water in which this pure quickſilver had been agitated acquired a peculiar ſmell and taſte, not eaſy to be deſcribed. When a pint of it was evaporated to dryneſs, there remained a ſmall quantity of matter, an account of which will be given hereafter. Common air agitated in this water was not ſenſibly diminiſhed, and therefore I conclude not ſenſibly injured by it.

Spirit of wine ſeems to anſwer this purpoſe as well as water, but not oil of turpentine. I expoſed them, together with various other things, to continued agitation in a mill, for ſeveral months; but when the phial containing them was examined, neither the quickſilver, nor the oil of turpentine, was ſenſibly changed. Of theſe obſervations I ſhall give a ſeparate account, at the cloſe of this article.

Hitherto I was intirely ignorant of the real nature of the black powder into which mercury is converted by agitation in water, and rather took it for granted that it was a partial calcination of that metal; though I might have recollected, that no ſuch thing as this black powder occurs in any part of the proceſs of a proper calcination of mercury, in converting it into *precipitate per ſe*. Nor did I at length diſcover the real nature of it by any reaſoning or conjecture *a priori*. But having conſtantly obſerved (what it was impoſſible not to obſerve) that whenever I ſpilled any of the water containing the black powder, the moment it was dry it appeared in the form of white running mercury, alſo that the glaſs funnel I made uſe of, in pouring this black water into the phial, was always found white, with ſmall globules of running mercury, whenever I took it up, after an interruption in my experiments; I could not but conclude that this converſion of black into white mercury was effected by the *air*, and therefore I determined to have this proceſs performed in *confined air*, in order to judge how the air itſelf was affected by it.

Accordingly

Accordingly, having a confiderable quantity of this black powder in a little water, enough to prevent its becoming running mercury, I poured fome of it into a fmall retort, and evaporating all the water that was mixed with it, while the neck of the retort was plunged in water, and admitting as little air as poffible (barely enough to prevent the retort from breaking by the rufhing in of frefh water, after the bulk of the air had been expelled by the heat and vapour within the retort) I examined the inclofed air when the veffel was cold; and found it to be worfe than common air. For one meafure of this and one of nitrous air occupied the fpace of 1.31 meafures; when one meafure of the common external air and another of the fame nitrous air occupied the fpace of 1.27 meafures.

It was evident, however, that, in this experiment, the air could only be very partially affected by the change of the mercury; fince a great deal muft neceffarily have been admitted after all the heat had been applied; and the newly admitted air muſt, of courſe, have diluted that which had been affected by the proceſs. I therefore made the following more deciſive experiment, which perfectly agreed with, and confirmed, the preceding.

I took a glaſs tube, about eighteen inches long, and half an inch wide, and pouring into it a quantity of the water and black powder of mercury, turned it every way till it had got a black coating in all places. I then inverted it, and placed it in a cup of water near the fire, but not ſo near as to convert the water within the tube into ſteam, and thereby expel too much of the air. In this ſituation I perceived, after ſome time, that the quickſilver was revivified, all the tube to which the heat had reached having now got a white coating, and having the appearance of a looking-glaſs. I then examined the air in the inſide of the tube, and found it to be very ſufficiently phlogiſticated. For one meaſure of it and one of nitrous air occupied the ſpace of 1.66 meaſures, notwithſtanding a conſiderable part of the tube

had

had not been ſo much heated as to have all the mercury on it revivified. I repeated this experiment in another tube, and with the ſame reſult, the air contained in it being as much phlogiſticated as before. At this time the tube being expoſed to too great a degree of heat part of the mercurial coating was partially calcined.

After this it was impoſſible to entertain a doubt concerning the nature of this black powder. It was evidently *mercury ſuper-phlogiſticated*, or which had acquired more phlogiſton than was neceſſary to its ſtate of white running mercury. But it remained to be inquired whence the mercury could have received this phlogiſton. That it might have been communicated from the ſpirit of wine in the experiment mentioned above was probable enough, becauſe ſpirit of wine is known to contain phlogiſton in abundance. But it has been a maxim with chemiſts, that water is incapable of forming any union with phlogiſton; and that beſides air, it is perhaps the only ſubſtance in nature that is incapable of it. However, as the whole courſe of my experiments has demonſtrated the fallacy of the maxim with reſpect to air, ſo I think alſo it has already appeared from them, that neither does it hold good with reſpect to water. For if water did not, in its natural ſtate, contain phlogiſton, how could pure air become phlogiſticated (which I think I have demonſtrated to be the caſe when it is rendered noxious) by agitation in it? And if it was incapable of receiving more phlogiſton than it naturally has, how could noxious air of all kinds be rendered wholeſome, or dephlogiſticated by agitation in it? Alſo that water and phlogiſton may form a *temporary union*, or be *mixed*, is, I think, evident from various obſervations, as from what is called the *empyreuma* of water freſh diſtilled; and eſpecially from the ſmell given to water by calcination of metals over it, in the experiments recited in my firſt volume. I am therefore, ſometimes inclined to think that, during the agitation of quickſilver in water, the water communicates of its phlogiſton to the mercury.

The

The moſt material objection that I am now aware of to this concluſion is, that the ſame water may be uſed, as far as I know, without end, and yet it will always retain, and even undiminiſhed, its power of converting mercury into this black powder. I once agitated mercury in a quantity of water very many times (having nothing but the mercury and the water in the phial) and after every proceſs I ſhut up the water from all acceſs of the external air; at the ſame time that another quantity of the ſame diſtilled water, agitated with mercury juſt as much, was always kept in an open veſſel; yet, after ſome time, when I repeated the agitation of the mercury, I was not able to perceive any difference between the effect of the agitation in the water that had been confined, and in that which had been expoſed to the air. Alſo, when I have examined the air confined in a phial over the ſurface of water in which mercury had been agitated a long time, it never appeared to be in the leaſt different from the common external air, as might have been expected, if the water had been deprived of any portion of its proper phlogiſton, and had recovered it again. For whence could it have been recruited, but from the air? It is alſo unfavourable to the hypotheſis above-mentioned, that water ſhould much more readily contribute to the converſion of mercury into black powder when it has been ſeveral times uſed for this purpoſe, than at the firſt.

Some may, therefore, think that it is the *calx* of the mercury that the water ſeizes upon, leaving the phlogiſton as an over charge upon part of the remainder. Whichever of theſe hypotheſis is the true one, it is a fact, and certainly a very remarkable one, that, if the water be warm, though only about blood heat, no agitation of mercury in it will convert it into black powder. And alſo, if the water be ever ſo black with the powder, the mere heating of it, without any acceſs of the external air, will make it tranſparent again; the blackneſs totally diſappearing both from the water and the mercury. If the former hypotheſis hypothefis be admitted, *viz.* that the over-charge of phlogifton is communicated from the mercury to the water, water muft be of fuch a nature as to have a ftronger affinity with phlogifton when hot than when cold, in which, though it be the reverfe of moft other fubftances, it has the fame property, however, with *air*, which receives phlogifton from ignited bodies, when both the air and the ignited body muft, of courfe, be equally hot.

Or, laftly, it may be fuppofed (and fome obfervations that will be recited hereafter prove this is actually the cafe in fome circumftances) that during the agitation one part of the mercury becomes dephlogifticated while another part is fuper-phlogifticated, extraordinary as the fact will be thought.

I obferved the effect of warm water in mercury in the following manner. Sitting pretty near the fire, when the weather was cold, I found that my agitation of the mercury had not fo much effect as it had been ufed to have; and, reflecting upon the

the ſubject, it occurred to me, that poſſibly it might be the *warmth* of the water in the phial that obſtructed it. To try this, I put the phial containing the water and the quickſilver into a pan of water, which I made to boil; after which I took it out of the pan, and holding it with a couple of handkerchiefs, agitated it with as much violence as I poſſibly could, but I found that I might do this as long as I pleaſed, without producing any thing like the black powder.

To complete this experiment on the effects of heat, as ſoon as the whole was cold, I ſhook the phial again, till the water was to appearance, almoſt as black as ink, and placed it in the water over the fire; obſerving that the phial was completely filled with the water and quickſilver, and that all air was excluded, only leaving the ſtopper rather looſe, that the expanſion of the water by the heat might not burſt it. The effect was that, preſently after the water in the pan began to boil, the water in the phial had recovered its tranſpa-

tranſparency; and when I examined the quickſilver, there was no appearance of black powder upon it. The whole had been reconverted into white running mercury, and had united with the reſt of the maſs of mercury in the phial. Alſo, when it was cold, the blackneſs did not re-appear; but the mercury was, in appearance, in the very ſame ſtate, as at the beginning of the proceſs.

This fact being a very remarkable one, I repeated the experiment many times, and in a very great variety of ways, but always with the ſame reſult. When, indeed, I agitated the mercury in the ſame water a very long time (and I once did it on purpoſe a quarter of an hour, with little or no intermiſſion, though in one minute I could make the water quite opake with the ſame degree of agitation) I have found that it requires a longer continuance of heat to make it perfectly tranſparent, and a ſlight blackneſs has remained on ſome parts of the ſurface of the quickſilver. But then this was quite trifling compared with the

quantity

quantity of black matter that lay upon it before it was heated, fo that by much the greateft part of the phlogifton muft have been abforbed.

Alfo, when I have poured the quickfilver off, and heated the turbid water by itfelf, the blacknefs has never failed to difappear. But fometimes a few globules of quick-filver would remain at the bottom of the phial, fome white, and others black; but though the latter were more numerous, they were probably only fuperficially black, and no agitation of the phial would ever give the water the turbid appearance that it had before; the globules, though dif-perfed through the water by the agitation, fubfiding in a moment, and falling, like fo many leaden fhot, to the bottom of the phial; fo that the black furface of thefe larger maffes of quickfilver, as they may be called, was very fmall in proportion to the furface of the infinite number of black molecules which conftituted the clouds of attenuated mercury that before had filled the

the whole phial, and made all the water in it opake.

That other perſons may more eaſily ſucceed in this experiment, I muſt inform them, that I have generally made uſe of a ten ounce phial, about a quarter of it filled with quickſilver, and the reſt with diſtilled water, ſhaking it as violently as I can, generally giving it ten or a dozen ſhakes in quick ſucceſſion, in the manner deſcribed above; and then waiting till the water and quickſilver be ſeparated from each other, which gives me a ſufficient interval of time to reſt from my labour.

The above-mentioned experiments may be made, with the ſame reſults, by ſubſtituting *ſpirit of wine* for water. After agitating ſome quickſilver in ſpirit of wine till it was very turbid, I placed the phial containing them in a pan of water, and preſently after it had boiled all the blackneſs diſappeared. Agitating it again, when it was hot, had no effect; and when it was cold the blackneſs did not return. The black powder thus procured became running

mercury

mercury when it was dry, but it was not ſo bright as that which had been agitated in water. Letting a quantity of it remain ſix or ſeven hours upon a plate of glaſs, on the iron plate of a Bath ſtove, in which there was a pretty good fire, it loſt its metallic luſtre and conſiſtence and became a *white powdery ſubſtance*, which was completely diſſolved in ſpirit of ſalt, and thereby appeared to be a perfect calx of mercury, though it was not brought to the ſtate of *precipitate per ſe*.

Having now advanced another ſtep in my inveſtigation of the changes of mercury, in paſſing from the *ſuper-phlogiſticated* to the dephlogiſticated ſtate; I went through the ſame proceſs with the black powder procured by the agitation of mercury in water, covering with it the greateſt part of the ſurface of a watch glaſs (which I find a very convenient thing for many ſmall experiments) and placing it on the plate of the Bath ſtove, very near the fire, ſo that different parts of it might be expoſed to different degrees of heat. The reſult of this

this experiment was very ſatisfactory and pleaſing.

At firſt, as I have obſerved before, the black powder became running mercury; but preſently after it adhered pretty firmly to the glaſs, and then, looking on the back ſide of it, I found it made the moſt perfect mirror imaginable, a better, I ſhould think, than that which is made with mercury and tin. With a longer expoſure to the ſame heat it loſt its metallic luſtre, and became a *white* powdery ſubſtance; and with more heat it aſſumed a *brown* colour. Yet a quantity of this brown matter, though, I doubt not, it was an approach to a proper *percipitate per ſe*, was not wholly diſſolved in ſpirit of ſalt, ſo that the calcination had been imperfect.

It was pleaſing to obſerve the mercury within ſo ſmall a compaſs as that of a watch glaſs, in three of the ſtates above-mentioned, *viz.* the white metallic ſtate, the white calx, and the brown, or the dephlogiſticated ſtate. On a larger plate of glaſs

all

all the four ſtates might have been exhibited in their natural order, the black powder, or the ſuper-phlogiſticated ſtate, preceding the reſt. The order in which theſe different ſtates of the mercury ſucceed each other is a proof of the hypotheſis I have advanced on the ſubject.

I ſhall now mention ſome other circumſtances relating to the agitation of water in mercury, the cauſes of which I own I do not underſtand, and ſome of them ſeem to militate againſt the hypotheſis advanced above. But this gives me no particular concern.

Indeed the greateſt difficulty ariſes from the fact mentioned above, *viz.* that water which has been often uſed in this proceſs has a much quicker and greater effect than water that is uſed the firſt time. This is more eſpecially the caſe with water that has been diſtilled a long time. This certainly proves, that ſome change has been made in the water as well as in the quickſilver. But if the water communicates phlogiſton to mercury, it might be expected that it would give it more readily at firſt than afterwards.

Alſo, if it be water that communicates the phlogiſton to the mercury, it might be expected that water freſh diſtilled would have a greater effect, on account of the empyreuma that it is ſuppoſed to acquire by diſtillation, and which is known not to leave it of a conſiderable time. And, in general, I have found that water freſh diſtilled ſooner becomes turbid in this proceſs than water that has been long diſtilled, but not ſo ſoon as water that has been often uſed for this purpoſe. Alſo when I have re-diſtilled water that has been much uſed in theſe experiments, it has been as readily affected as before the ſecond diſtillation; but with this difference, that the black powder has been much longer in ſubſiding than it had been before.

I have often found great differences in water in this reſpect. In general, if the water has been long diſtilled, and frequently uſed, the depoſit will be completely made in a few minutes; whereas I have sometimes found that the water has not become clear (using the same mercury) in three or four days. And even when the blackness has disappeared, a white cloudiness will remain I do not know how long.

I found some difference, but not so much as I had expected, between water distilled with a gentle heat without boiling, and water that was made to boil violently during the distillation, though both were distilled in glass vessels. They both became turbid pretty soon, and the quantity of black powder was nearly equal from both, in the same time; but that which had been hastily distilled deposited its sediment in about ten minutes, whereas the other had done it very imperfectly in an hour. I would not, however, be positive that a second experiment of this kind would have a similar result, as this circumstance may depend upon a cause not yet investigated.

There was something remarkable in the phenomena that occurred in using a quantity

tity of water fresh distilled in a copper vessel, and a pewter worm, in the common way; but in which some *elder flowers* had been distilled about a year before, so that the water had a slight smell of it. But whether this circumstance has any thing to do with what I am going to describe, I cannot tell.

Agitating the quicksilver in this water, it presently became very turbid, but the sediment was not deposited in a week, or indeed completely, in a fortnight; and then the water retained a white cloudiness. But the most remarkable circumstance was that, in agitating the mercury in this water, the whole mass was presently divided into small globules, not larger than the smallest pins heads, and did not very readily unite again. Several times I have found that the mercury thus divided would choak up the mouth of the phial, which is about half an inch wide; so that, holding it perpendicularly, it would not run out at all in several seconds. It has even required shaking to get it all out. It has then exhibited a singular and beautiful appearance in the cup into which the phial was emptied, whereas the very ſame quickſilver agitated in other water, immediately before, and after, has been attended with no other than the common appearance. It was alſo remarkable, that this divided maſs of mercury, after the moſt violent agitation in the water, fell inſtantly to the bottom, like a quantity of leaden ſhot; whereas, in general, as I have obſerved, the mercury and water get intangled in ſuch a manner, that they do not intirely ſeparate in ſeveral ſeconds.

Imagining at firſt, that the power of re-union, in the divided mercury, might perhaps have been impaired by ſome effect of the ſmall remains of the elder water, mixed with the freſh diſtilled water in which it was now agitated, I made trial of *mint water*, but without any ſuch effect. A conſiderable time afterwards, however, I found other methods of producing the ſame effect, and even in a much more remarkable manner, though I am ſtill at a loſs to account for the *proximate cauſe* of the phenomenon.

Having a phial containing ſome water imper-

imperfectly impregnated with vitriolic acid air, and likewise a quantity of the quicksilver on which the impregnation had been made, I found that when they were agitated together the whole mass of quicksilver was divided into small globules, and that they did not perfectly reunite after being at rest a day and night. But when the phial was *heated*, they united as readily as in common water. When it was cold again, the mercury was divided by agitation, and continued divided, but not quite so much as before.

This being an *acid* liquor, I made trial of other acids; and I found the same effect with *oil of vitriol*; but the division of the mercury into small globules did not continue very long, and when it was hot the effect was inconsiderable. But the most complete effect of this kind is produced by *vinegar*. A very little agitation of mercury in this acid divides it into the smallest globules; and they continue without any apparent disposition to reunite even when very hot. While this divided mercury is in the vinegar the globules may be poured from one part of the phial to the other exactly like fine dry ſand, and they exhibit a ſingular and beautiful appearance. All the vinegar muſt be evaporated by heat before theſe globules will unite.

Mercury agitated in ſpirit of ſalt, and alſo in a volatile alkaline liquor, was not attended with any remarkable appearance of this kind.

I have ſometimes been much amuſed with another ſingular appearance. In agitating mercury in water, eſpecially when freſh diſtilled (when there has not been a bubble of air in the phial) large balls of various ſizes, ſome not leſs than half an inch in diameter, have not only rolled upon the ſurface of the mercury, after it had completely ſubſided, and continued there a conſiderable time, but have floated up and down in the water, like ſope bubbles in the air. Theſe bubbles muſt conſiſt of water incloſed in a thin pellicle of mercury, for when they burſt, nothing viſible comes out of them, and the quantity

quantity of mercury about them is not enough to be perceived in its defcent through the water afterwards.

I may alfo mention, as another pleafing phenomenon in thefe experiments, the viewing of a fmall quantity of the moiften-ed black powder with the microfcope. For in the inftant that it becomes dry, the colour changes; and in fo fmall a quantity the change is almoft inftantaneous, fo that the black globules immediately become white, and beautifully polifhed ones.

In order to afcertain what change had taken place in the *water* in which mercury had been agitated, I diftilled a quantity of it, and the refult of the experiment is rather in favour of the water having feized upon the calx of the mercury, than of its having parted with any phlogifton to it.

After the diftillation I found a confider-able quantity of a yellowifh refiduum, which, when it was expofed to heat, on a plate of glafs, became quite *black*, and with more heat was *brown*. Being expofed to the open air, it became very moift. Putting it, after this, into a glaſs tube, and expoſing it to a red heat, a whitiſh matter ſublimed from it, and coated the inſide of the tube at ſome diſtance from it. This matter was not diſſolved by ſpirit of ſalt; and therefore, though I think, from the appearance of it, it was probably a calx of mercury, it muſt have been an imperfect one, containing a conſiderable proportion of phlogiſton.

SECTION XVIII.

Of the Effect of long continued Agitation on Quickſilver.

IN order to give quickſilver, in conjunction with various other ſubſtances, a much more, and a longer continued agitation, than I was able to give them by ſhaking the phials that contained them in my hand, I got a ſtrong wooden box, and had a contrivance in a neighbouring mill to have it agitated whenever the mill was in motion, which I found was,

at

at a medium, about twelve hours in twenty-four. There was ſome difference in the circumſtances of the quickſilver in all the veſſels, and I ſhall give a brief account of what I obſerved with reſpect to them. The box was made up, and ſent to the mill on the 9th of December 1777, and the contents of it were examined on the 10th of May following.

No. I. An eight ounce phial with a ground ſtopper containing a pound of quickſilver, except 5 dw. which it had loſt by frequent agitation in the ſame diſtilled water with which it was now ſhut up, the water being about four times the bulk of the quickſilver, marks being made upon the phial with a file, to denote the height of the water and of the quickſilver. When it was examined, the water appeared to have been diminiſhed one ſeventh in its bulk, having poſſibly made its eſcape by the ſide of the ſtopper. The quickſilver had loſt eighteen grains, which was probably the weight of the black powder that was formed in it; but what I thought the

moſt

moſt extraordinary circumſtance, was that the bottom of the phial was tinged with a deep orange colour. Not willing to put other water, or other quickſilver into this phial, I made no other trial of the air, than by letting a ſmall candle down into it, and I obſerved that, to all appearance, it burned very well.

No. II. A glaſs tube hermetically ſealed, containing quickſilver and diſtilled water, which had been agitated one month before, in conſequence of which a good deal of black powder had been formed. This had received an increaſe of black powder, and part of the veſſel was coated with the brown matter above-mentioned.

No. III. A three ounce phial with a ground ſtopper, containing quickſilver and water diſtilled in glaſs, about twice the bulk of the quickſilver. The ſurface of the mercury was well covered with black powder, and beſide this, a good deal adhered to the bottom of the phial, and was alſo diſpoſed in ſtreaks almoſt ſurrounding it, in the middle of that part of the phial

that

that had been occupied by the mercury. This black coating, viewed in a certain light, appeared of a dirty orange colour. A candle burned in the top of the phial.

No. IV. A three ounce phial with a ground ſtopper, about one fifth filled with quickſilver, without water. The quickſilver was well covered with black powder, and alſo a great part of the inſide of the phial. A candle burned in it very well. Whether this quickſilver was perfectly pure I cannot abſolutely ſay. If it had, I can hardly think there would have been ſo much black powder; and yet had it been very impure, the air within the phial would have been phlogiſticated. If the quickſilver was pure, the agitation muſt have diſpoſed one part of the quickſilver to part with its phlogiſton, and another part of the ſame maſs to have received it, which the circumſtances in the other caſes render probable; and if we admit this hypotheſis, we ſhall be relieved from the ſuppoſition of the water, in the former experiments, communicating the phlogiſton to the quickſilver,

quickſilver, in order to the formation of the black powder.

No. V. A two ounce phial with a ground ſtopper, containing quickſilver and ſpirit of wine, the latter one and a half more in bulk than the former. The ſpirit was a little diminiſhed in bulk, the mercury had more black powder upon it than there was in the phial containing quickſilver and water, and a compact body of this black powder covered one ſide of the phial; beginning at the ſurface of the ſpirit, and reaching to the top.

No. VI. A tall green glaſs phial, with a little quickſilver and diſtilled water, and a green glaſs peſtle, weighing 9 dw. 4 gr. The phial was coated with black powder, a candle burned in the phial, and the peſtle weighed 9 dw. $\frac{1}{4}$ gr.

No. VII. A tall green glaſs phial, containing quickſilver 7 oz. odw. 12 grs. lead 2 dw. with diſtilled water. A candle would not burn in the phial, it was coated with black powder, but there was very little of it, notwithſtanding the maſs of

mercury

mercury and lead had not loſt quite two grains of their weight.

No. VIII. A two ounce phial with a ground ſtopper, containing mercury and oil of turpentine, about one and a half as much as the bulk of the mercury. In this there was no ſenſible change.

I have obſerved that, in the phial in which quickſilver only had been agitated, and alſo in another which had contained both quickſilver and water, there was a quantity of browniſh matter adhering to the glaſs. Had this matter been a calx of lead, mixed with the mercury, the air within the phial would certainly have been phlogiſticated. Beſides I am pretty ſure that I had taken ſufficient care to have this mercury pure. I am therefore inclined to think, notwithſtanding the peculiar manner in which it was produced, that it was the *precipitate per ſe*. The few obſervations that I did make upon it are all in favour of this ſuppoſition. When I expoſed it to the heat of the fire, it became of a deep and proper orange colour, and when I expoſed the

the phial that contained it to a great degree of heat, but not fufficient to melt the glafs, the air within the phial was found afterwards to be rather better than common air, though not fo much as that I could be abfolutely certain the feeming difference might not have been owing to fome accident in making the experiment.

But what I think the moft nearly decifive in favour of this hypothefis is, that the phenomena attending the folution of this fubftance, and of the *precipitate per fe*, in fpirit of falt, are, in all the refpects in which I compared them, the very fame. This orange coloured matter in the phials was inftantly diffolved by the fpirit of falt, which, from being of a light ftraw colour, became colourlefs, like water; and when it was afterwards evaporated, it left a perfectly white fubftance behind. In all thefe particulars the folution of a fmall quantity of *precipitate per fe* was attended with the fame appearances. Alfo when a little of both the *refidua* was laid on a thin plate of glafs, and expofed

to

to the heat of a candle, they were evaporated in a white ſmoke, exactly alike.

Admitting this ſubſtance to be a true *percipitate per ſe*, or a complete calx of mercury, we may perhaps explain the formation of the *black powder* produced by the agitation of mercury in water, by ſuppoſing that while one part of the mercury is ſuperphlogiſticated, and becomes black, another part of the ſame maſs is dephlogiſticated or reduced to a calx, which is firſt white, but would in time aſſume an orange colour. And that the water diſſolves a part of this calx, ſeems probable from the obſervation I made on the depoſit made by it when it was evaporated.

SECTION XIX.

Of the Conſtitution of dephlogiſticated Air, and a Review of the Obſervations relating to it.

ON the ſubject of *dephlogiſticated air*, I am happy to be able to give my readers much more light than they, or I, could have any reaſon to expect from my laſt publication relating to air, eſpecially with reſpect to the fundamental articles of the *origin*, and conſequently the *conſtitution* of it, and therefore of the atmoſphere in which we live. As it ſometimes amuſes myſelf, it may perhaps amuſe others, to look back with me to the ſeveral ſteps in the actual progreſs of this inveſtigation, ſome of which I over-looked in my laſt account of it.

From a view of my different publications relating to air, it will appear, that I was, in fact, poſſeſſed of this remarkable ſpecies of air at a very early period in my inquiries, as may be collected from my firſt

first papers sent to the Royal Society, and before I formed them into a volume. For the peculiar characters of this kind of air will be found in my description of that which I extracted from *salt petre*, and also of that from *alum*. See my *Observations on Air*, Vol. I. p. 155. I there say "All kinds "of factitious air, on which I have yet "made the experiment, are equally noxi-"ous, except that which is extracted from "salt petre, or alum; but in this even a "candle burned just as in common air. "In one quantity which I got from salt "petre a candle not only burned, but the "flame was increased, and something was "heard like a *hissing*, similar to the de-"crepitation of nitre in an open fire. This "experiment was made when the air was "fresh made, and while it probably con-"tained some particles of nitre, which "would have been deposited afterwards." This *hissing*, however, was certainly owing to the avidity with which this pure air seized upon the phlogiston of the bodies ignited in it; and that property would not have been loſt by mere *keeping*, as I then imagined.

This air, it appears that I was in poſſeſſion of before the month of November 1771. For in November 1772 I mentioned my examining a quantity of air which had been extracted from ſalt petre *above a year* before, and which I ſuppoſed to have, by ſome means or other, become noxious, but to have been reſtored to its former wholeſome ſtate, ſo as to efferveſce with nitrous air, and admit a candle to burn in it, in conſequence of agitation in water. "This ſeries of facts," I then ſay, p. 157 "relating to air extracted from nitre appears "to me to be very extraordinary and important; and in able hands may lead to "conſiderable diſcoveries." Conſiderable diſcoveries, indeed, have been made on the ſubject ſince that time, but not in conſequence of theſe firſt hints falling into *able hands*; for the whole has been a ſucceſſion of very extraordinary *accidents*.

So far was I from having a right idea of the nature of this air; or rather, ſo far

was

was I from preserving a right idea of it (for it is evident that, at the first, I considered it as having the valuable properties of common air, at least) that when I republished my first volume, which was about a year afterwards, I added a note to the above account of air extracted from nitre, in which I say, " It is probable that, though " a candle burned even more than well in " this air, an animal would not have lived " in it, though, at the time of my first " publication, I had no idea of this being " possible in nature." This doubt was suggested by my having afterwards brought nitrous air into a state in which a candle burned in it with a natural, or even an enlarged flame, though it was still noxious; not having attended to the difference between the appearance of that *enlarged flame*, and that of the peculiarly *vivid flame* of a candle in dephlogisticated air, in consequence of the experiments having been made at a great distance of time from each other.

In this state the matter rested till August 1774; when, without any particular view, except that of extracting air from a variety of ſubſtances by means of a burning lens in quickſilver, which was then a new proceſs with me, and which I was very fond of, I extracted this air from the *precipitate per ſe*, from the common *red precipitate*, and with a mixture of fixed air, from *red lead* alſo. See my *ſecond volume*, p. 34, &c.

In this air, I obſerved that a candle burned with a remarkably vigorous flame; but I did not even then attend to the difference between this appearance, and that of the enlarged flame above-mentioned. This air, however, having been procured without nitre, puzzled me exceedingly, and I could not even believe that my precipitate was a right preparation of the kind, till I procured a quantity of Mr. Cadet at Paris, when I was there in the October following. But from this preparation, in the March following, as will be ſeen in my *ſecond volume*, p. 40, I got air which I was gradually ſatisfied had all the properties of common air, only in much greater perfection, ſo as to be intitled (according to my my idea of purity and impurity with respect to air) to the name of dephlogisticated air, which, for that reason, I gave to it.

My idea of this kind of air, and consequently of atmospherical air (which is the same thing, but in a state of inferior purity) consisting of earth and spirit of nitre arose in the following manner. Having procured inflammable air from various substances by the help of the marine acid air, and being able to render this inflammable air fit for respiration, I conjectured that one ingredient in the composition of this air was *that acid*, and that the great mass of air belonging to this planet might have been originally supplied by volcano's, which I supposed to throw out a great quantity of inflammable air.

Having now got pure air from *red lead*, which I supposed to have acquired its power of yielding it, in consequence of having imbibed some acid from the air; and having fortunately procured a quantity of red lead in such a state that it would not *of itself* yield any, or very little air, I moistened ſeparate portions of it with each of the three mineral acids, in order to determine which of them it was that it had imbibed. And preſently finding that the portion which had been moiſtened with the nitrous acid yielded plenty of the ſame kind of air that red lead in its natural ſtate yields, and that the portions which had been moiſtened with the other acids yielded none at all, I had no doubt but that it was the nitrous acid that it had imbibed before. I was farther confirmed in this opinion, by being able to procure dephlogiſticated air by the mixture of nitrous acid with any kind of earth whatever; ſo that there is not, I believe, any ſubſtance in nature, which I am not able, by the help of this acid, to convert into air.

Such were my ideas at the time of the publication of my ſecond and third volumes on the ſubject of air. But I have ſince ſeen reaſon to ſuſpect that hypotheſis, plauſible as it appears; and at preſent I am inclined to think that, though, beſides *earth*, ſome acid enters into the compoſition

of

of air, it is not neceſſarily the *nitrous* acid, but, in ſome caſes, the *vitriolic*; or at leaſt, in the proceſſes by which this air is procured, they are converted into one another, or into ſome other acid, or ſubſtance, that bears an equal relation to them both; and that, in this ſtate, common to them both, it exiſts in the atmoſphere.

Indeed, ſome of my late experiments would lead me to conclude, that there is no acid at all in pure air, but that thoſe which I made with the ſolution of mercury in ſpirit of nitre, mentioned in the preface to my third volume, ſeem to be deciſive in favour of the contrary. For though, in my opinion, they prove that ſome earth enters into the compoſition of air, or, at leaſt, that earth is diſſolved in air, yet the actual weight of the air procured in the proceſs greatly exceeds the loſs of weight ſuſtained by the mercury; ſo that what the air weighs more than that loſs it muſt neceſſarily, as I think, have derived from the acid.

The new facts that have occaſioned this fluctuation in my opinion on this ſubject are, that pure air is extracted from ſubſtances combined with the vitriolic acid, from various mineral ſubſtances, which we do not know to have had any communication with the atmoſphere, and other remarkable facts of a ſimilar nature. Theſe *new facts* I ſhall exhibit, as nearly as I can, in the order in which they occurred to me.

I was led to thoſe relating to the vitriolic acid, and mineral ſubſtances, in a courſe of experiments made with no other view than ſimply to try what kind of air, and in what proportion, ſuch ſubſtances as I could conveniently meet with would yield. And it is ſomething remarkable, that though I have mentioned my having got pure air from *Roman vitriol*, in one of the moſt accurate of all my proceſſes, I ſhould not have given more attention to the obſervation, and have purſued it farther; but that, after ſo long an interval of time, I ſhould have been brought partly by accidents reſpecting my own experiments, and partly by the aſſiſtance of others, into the very track which my own former obſer-

observations could not have failed to bring me.

The discovery of the vitriolic acid contributing to the production of dephlogisticated air properly belongs to Mr. Landriani, and I had the communication of it from him. He informed me, that he had procured this kind of air both from *turbith mineral*, and also from *corrosive sublimate*. The latter I tried immediately upon the receipt of his letter, but I was unable to procure any air from it; and finding that nitrous acid was sometimes used in the preparation of *turbith* mineral, I imagined that he might possibly have made use of that kind of preparation, and that with respect to the corrosive sublimate, he had made some mistake that I could not discover. I therefore contented myself with pursuing the track that I was then in; and having first found dephlogisticated air in manganese, and other mineral substances, I afterwards, in continuing the same process, with the same general and indistinct views procured it from *green copperas*, and at length from

from other vitriolic ſalts; and it was not without great difficulty that I could be brought to believe that it was the *pure vitriolic* acid that was the proper cauſe of this effect.

In this introductory ſection I would likewiſe obſerve, that, in making theſe experiments, I generally made uſe of ſmall bellied retorts, containing about an ounce of water, with very long and narrow necks, *viz.* of eighteen or twenty inches, putting the ſubſtance on which I made the experiment into them, and then expoſing them to a red heat, either in ſand, or over a naked fire, while the neck of the retort was plunged in water or mercury.

The reaſon why I had no better ſucceſs when I endeavoured to procure air from many of the ſame ſubſtances before, was either my trying them only in very ſmall quantities, upon quickſilver, with the heat of a burning lens, or uſing a gun barrel, the phlogiſton from which contaminated the air. I did, indeed, ſometimes make uſe of a phial with a ground ſtopper and long tube;

tube; but this being an expensive instrument, I used it very rarely, and it was more liable to accidents than the long retorts, which are reasonably cheap, especially when made of green glass; which is, at the same time the best in other respects, as containing no lead, and able to sustain a greater degree of heat.

SECTION XX.

Of the Extraction of Dephlogisticated Air from several Mineral Substances.

WITH no other view than the general one mentioned above, *viz.* to try what kind, and proportion of air, different substances would give in a red heat, I entered upon the examination of *manganese*, about which much has been written, but of which much yet remains to be investigated by chemists. Of this I procured a quantity finely pounded; and from an ounce of it I got, in a red sand heat, forty ounce measures of air, part of which, in every portion,

portion, was fixed air, and at firft almoft wholly fo; but four fifths of the laft was the pureft dephlogifticated air. Even the firft that came over, which was the common air, in the veffel was not in the leaft dephlogifticated. The manganefe had loft $1\frac{1}{2}$ dwts. of its weight, and was not to be diftinguifhed in colour (which was black) from what it had been before. A confiderable quantity of water came over during this procefs.

That Manganefe fhould give *fixed air* did not at all furprize me; fince there are few earthy fubftances that do not contain more or lefs of it; but I did not at all expect the dephlogifticated air; as, before I had imagined that the nitrous acid was neceffary to the production of it, or at leaft the influence of the atmofphere, which I fuppofed might depofit the acid that entered into its compofition, and which I concluded to be the nitrous. On the contrary here was pure air from a fubftance which, for any thing that appeared, had always been in the bowels of the earth, and never had had any communication with the external air; and yet it exactly resembled *red lead*, both in yielding fixed air, and dephlogisticated air; and it is known that red lead, like the *precipitate per se* cannot be made but in contact with the open air.

In order to see how much more air this calcined manganese would yield with spirit of nitre, I moistened it with that acid; and in a red sand heat got from it about thirty ounce measures of air, part of which, at the beginning, was fixed air, but not afterwards. The residuum was at first strongly nitrous, but at the last two thirds of the whole was pure dephlogisticated air. The glass vessel in which the experiment was made had some black matter adhering to it, from a former process; and this might possibly have contributed to the production of the nitrous air, and consequently to the diminution and depravation of the dephlogisticated air. The mixture was made with considerable heat, and the substance was black afterwards.

To ſee what alteration the mixture of nitre would make with the manganeſe (a proceſs which I went through with a variety of ſubſtances, in order to find a cheap method of procuring dephlogiſticated air) I mixed one ounce of the manganeſe, and half an ounce of nitre, when I got 108 ounce meaſures of air, the fixed air about the ſame quantity as before, and the reſt de-phlogiſticated air.

I next made trial of ſome *lapis calaminaris*, firſt pounding it very finely, then putting an ounce of it into one of the ſmall long necked retorts already mentioned, and with a red hot ſand heat I got from it 306 ounce meaſures of air; and making allowance for the loſs of air in changing the veſſels in which I received it, &c. I believe I may ſtate the whole produce at 316 ounce mea-ſures, the whole of which was fixed air, except four ounce meaſures; and, what I did not at all expect, the reſiduum, after the fixed air had been extracted from it by water, appeared to be nearly as good as common

common air. For one meaſure of it and one of nitrous air occupied the ſpace of $1\frac{1}{4}$ meaſures. Had the proper reſiduum of fixed air been well extracted, the remainder would probably have been dephlogiſticated air. What remained of the lapis calaminaris weighed 13 dw. 6 gr. and had a lighter colour than before. This produce of air I took at different times, but the reſiduum of the laſt portion was but little better than that of the firſt.

From this experiment it appears that lapis calaminaris, as well as manganeſe, reſembles, in ſome degree, red lead, which of itſelf, by means of heat only, yields both fixed air and dephlogiſticated air, only a much ſmaller proportion of the latter.

In order to obſerve whether the addition of ſpirit of nitre would make any change in the produce of air, I moiſtened another ounce of it with that acid; and in a glaſs veſſel, and with a red heat as before, I got from it 244 ounce meaſures of air, the bulk of which was fixed air, but twelve ounce meaſures were not abſorbed by water, and appeared to be dephlogiſticated air.

It appears from this experiment, that the ſpirit of nitre, beſides contributing to the production of dephlogiſticated air, as uſual, contributed alſo to the production of a large quantity of fixed air, which favours the hypotheſis of fixed air being a modefication of the nitrous acid. It will be found, however, in the courſe of this work, that it is not this acid only, but the vitriolic acid alſo, that contributes to the formation of fixed air, as well as of dephlogiſticated air. *N. B.* The ſpirit of nitre made no effervefcence, and produced no heat in mixing with the lapis calaminaris. It alſo made no change of colour in it, and a very little of the acid was ſufficient to make it ſenſibly moiſt; in all which reſpects it differs very remarkably both from red lead, and from manganeſe.

I next proceeded to the examination of the mineral ſubſtance called *wolfram*, with a good ſpecimen of which the Rev. Mr. Townſend had been ſo obliging as to ſupply

ſupply me, from the mines of Cornwall. This I pounded, eſpecially the black part of it, and I treated it, in all reſpects, as I had done the lapis calaminaris. But with the ſame proceſs I procured from an ounce of it, not more than about an ounce meaſure of air, a little of which was fixed air, but the remainder was about the ſtandard of common air. It required a great and long continued heat to extract this air, and I had nearly deſiſted from the proceſs before any of it came. After the proceſs the wolfram was, to all appearance, the ſame as before. Perhaps a greater degree of heat, in veſſels proper for ſuſtaining it, would have produced a greater quantity of air.

Theſe experiments ſuggeſted to me that, poſſibly, the expulſion of dephlogiſticated air from theſe, and other mineral ſubſtances, might aſſiſt in ſuſtaining ſubterraneous fires. For phlogiſton ſet looſe in the diſſolution of all bodies by ignition muſt be received by ſome other ſubſtance, as it is not a thing that, as far as we know, can exiſt, except in combination with other ſubſtances; and we do not know of any thing that can combine with it ſo readily as *air*; and therefore we find that nothing can burn but in contact with air, and with change of air.

When, indeed, phlogiſton is ſet looſe in the putrefactive proceſs, air is not abſolutely neceſſary. For, in that caſe, it may be communicated to water, and probably to other ſubſtances fluid or ſolid. It does not, therefore, certainly follow, that there can be no combuſtion without air, though it be probable; becauſe phlogiſton may be able to eſcape without the help of air in one way, though not in another. The ſolution of the phenomena of ſubterraneous fires would certainly, however, be much eaſier on the ſuppoſition of their ſupplying their own *pabulum*, by means of dephlogiſticated air, contained in ſubſtances expoſed to their heat. I therefore deſired Mr. Landriani, who, being in Italy, had a good opportunity of making inquiries on the the ſubject, to inform me whether any of thoſe ſubſtances and particularly *mangneſe*, be be found in their volcanos; and his anſwer makes it rather probable that thoſe fires are, in part, ſuſtained by this means. The extract of his letter, tranſlated from the Italian, is as follows

" With reſpect to what you deſire to be " informed of, concerning the *volcanic pro-* " *ductions*, there is found in the zolfatara " of Pozzuolo a great quantity of *martial* " *vitriol*; but I do not know that there is " any mangaueſe, or lapis calaminaris, " found there. The Abbe Jortis, who has " lately examined the extinguiſhed volcano " of Verona, aſſures me that, beſides *mar-* " *tial vitriol*, he has found a quantity of " manganeſe there. Sig. Volta, having re- " peated the experiments that I commu- " nicated to him, has lately informed me, " that he has found dephlogiſticated air in " calcined *roche alum*, a ſubſtance which " is found in great quantities in all vol- " canos; ſo that it is out of doubt, that " ſubterraneous fires are continually fed " with dephlogiſticated air, diſlodged from " ſubſtances proper for ſupplying it."

 It

It is very probable, that other mineral ſubſtances may contain dephlogiſticated air as well as theſe; and it is certainly very well worth while to add this proceſs to the chemical analyſes of them. Whether the ſubſtance be converted into air, or whether it contain the air, in a condenſed or combined ſtate, like fixed air in chalk, it is ſtill of importance to know what kind of air they may be made to yield by heat; and in time we may be able to aſcertain the true origin of ſuch air. It is alſo of conſequence in order to diſcover the eaſieſt and cheapeſt method of procuring dephlogiſticated air in large quantities; ſpirit of nitre, or even crude nitre, being expenſive articles. And though oil of vitriol be much cheaper, it will be ſeen that the quantity of dephlogiſticated air procured by means of this acid is not conſiderable, except from mercury, which is alſo a dear article.

SECTION XXI.

Of the Production of dephlogiſticated Air from the Vitriolic Acid and Iron.

AS I have been, perhaps, more than any other perſon, indebted to what are commonly call *accidents* (I mean with reſpect to *us*; for, in the general plan of nature, and with reſpect to that great Being who conducts and appoints every thing, there cannot be any ſuch thing as accident) ſo have I been very often prevented by other accidents from making valuable diſcoveries, to which I had made near approaches. This was remarkably the caſe with reſpect to the production of dephlogiſticated air from ſubſtances containing the vitriolic acid. For had I, in what I improperly ſuppoſed to be an *experimentum crucis*, made uſe of the calx of perhaps any other metal beſides *lead*, on which the vitriolic acid has no proper action, I could not have failed to hit upon what the better genius of Mr. Landriani brought him acquainted with. Having, as I obſerved before, got a quantity of red lead which was in a ſtate to give little or no air of itſelf, I got pure air from it, in great abundance, by means of the nitrous acid, but none at all by means of the vitriolic or marine acids. I therefore concluded, that the nitrous acid, and not either of the other mineral acids, enters into the compoſition of dephlogiſticated, or atmoſpherical air.

Mr. Keir, who has given us an excellent tranſlation of *Mr. Macquer's Chemical Dictionary*, with very valuable ſupplemental notes, in a very uſeful *Treatiſe on Gaſes* (for ſo he chuſes to call the different kinds of air) has ſuppoſed that the oil of vitriol really contributes to the production of dephlogiſticated air from red lead. But he does not ſeem to have attended to the quantity of this kind of air that red lead will yield by heat only, without any acid; and after repeating the experiment with the greateſt attention, I do not find that any more air can be procured from the red lead

lead with the oil of vitriol than without it. He mentions, p. 28, his procuring 36 cubic inches of air from 48 dwts. of red lead. But from 2 ounces, or 40 dwts. of such red lead as I now use, I am able to get, by heat only, 24 ounce measures of air, which is almost 48 cubic inches. Mixing half the weight of oil of vitriol with this red lead, I got as nearly as possible, the very same quantity of air; and when I mixed oil of vitriol with red lead out of which I had by calcination expelled all its air, it yielded nothing but a very small quantity of fixed air.

It was not till after I had made the experiments before recited on manganese, and other mineral substances, that I thought of subjecting *green vitriol*, and other saline matters, to the same trial. It is true I had tried them before; but the method was not adequate to the purpose. And though I had even got a small quantity of air considerably better than common air from *Roman vitriol* (See vol. II. p. 86.) I had concluded that " there must certainly

 " have

"have been ſome nitrous acid in that "Roman vitriol." In this caſe, therefore, as in the experiments with *ſalt petre*, and *alum*, I had made a diſcovery without being ſenſible of the value of it, or indeed underſtanding it. Nor, when I reſumed my experiments on vitriol, had I any expectation of getting from it any thing beſides fixed air and water. However, having every thing at hand, a very ſlight motive was ſufficient to induce me to include this among other articles deſtined for the ſame proceſs.

In this manner, therefore, without expecting the actual reſult, on the 24th of November 1777, I put an ounce of green vitriol into a glaſs veſſel, and with a ſand heat got from it, at firſt, after the common air was expelled, and the vapour of the water combined with it was come over, a little fixed air; then, after ſome interval, a large quantity of vitriolic acid air; the reſiduum of which was at firſt hardly perceivable, but was afterwards conſiderable, and chiefly fixed air. When the reſiduum was ſtill more conſiderable, I found that it was diminiſhed

diminished by nitrous air, at length it had no mixture of vitriolic acid air, but was very turbid, and appeared to be pure dephlogisticated air, except that, at the last, it was not quite so pure as before, which I thought rather extraordinary. Of this dephlogisticated air, I collected ten ounce measures. What remained in the glass vessel was 6 dw. of a purplish coloured ochre.

On this residuum I poured a quantity of spirit of nitre, which mixed with it as it does with clay, without any sensible heat; and then it yielded two ounce measures of air, the greatest part of which was fixed air, and the remainder dephlogisticated, except a little at the last, which, contrary to what happens in most other processes, was phlogisticated, not being at all diminished by nitrous air. Probably, however, there may be a quantity of phlogiston so intimately combined with this ochre (and its deep colour makes this not improbable) that nothing but a great degree of heat can expel it; when it will, of course, vitiate the air that is generated at the same time.

Not-

Notwithftanding this evident production of a confiderable quantity of dephlogifticated air from green vitriol, which is a combination of iron and the vitriolic acid, I ftill fufpected, as in the cafe of the Roman vitriol mentioned before, that, by expofure to the common atmofphere, or in fome other unknown manner, this vitriol, which had been bought at a common fhop, might have got fome mixture of fpirit of nitre. I therefore made a quantity of vitriol myfelf by diffolving iron filings in oil of vitriol, diluted with water. This vitriol, treated as the former had been, yielded air of all the fame kinds, and in the fame proportions, as in the preceding experiment; the dephlogifticated air, as then, being very turbid, and exceedingly pure. The firft air that came over was the common air in the veffel a little phlogifticated. A very fmall quantity of fixed air was ftill obferved in the refiduum of the vitriolic acid air, but none after the dephlogifticated air was procured.

It now, however, occurred to me, that as nitre is usedin the common procefs for making oil of vitriol in large quantities, there might be a mixture of this, and in all the oil of vitriol of the common fort. I therefore, in the next place, made ufe of *Newman's* oil of vitriol, which I was informed was made in the old method, in which no nitre is ufed. With this I made fome green vitriol as before; and, diftilling it to drynefs with a fand heat in a glafs veffel, I got from it firft a confiderable quantity of phlogifticated air, then pure fixed air, but not much; and, laftly, neglecting the vitriolic acid air, pure dephlogifticated air, though in a fmaller quantity than before. But this I impute to my not having carefully feparated the vitriol that I had made from the iron filings that remained undiffolved in the diluted oil of vitriol. For the whole mafs that I made ufe of was of a dark colour, containing much iron, mixed with the cryftals of the vitriol.

In making the vitriol for all the abovementioned experiments, I had taken care that

that the cryſtals ſhould be formed at the bottom of a deep glaſs veſſel, ſo as to have no viſible communication with the external air; and I had alſo covered the veſſel as carefully as I could during the proceſs, and had ſpent as little time as poſſible in conveying the vitriol from the veſſel in which it was formed into that in which it was to be diſtilled. I determined, however, to avoid the ſmall objection to which this trifling expoſure to the air was liable, and therefore next made the diſtillation in the ſame retort in which the ſolution had been made, and in the continuation of the ſame proceſs, ſo that all communication with the external air was moſt effectually precluded.

For this purpoſe I diſſolved 6 dw. 4 gr. of iron in diluted Newman's oil of vitriol, and diſtilling to dryneſs in a retort with a long neck, I got from it, after the common air was expelled, a ſmall quantity of fixed air, a prodigious quantity of vitriolic acid air, and likewiſe about 22 ounce meaſures of the pureſt dephlogiſticated air.

With

With more heat, I believe more of this air might have been procured. The common air that came over at this time was not at all phlogiſticated. When I examined the reſiduum, I found remaining 1 dw. 15 gr. of iron undiſſolved, ſo that the 22 oz. meaſures of dephlogiſticated air had been yielded by 4 dw. 13 grs. of iron.

Being informed by ſome of my chemical friends, that, probably, there is more or leſs of ſpirit of nitre in all oil of vitriol, when it is firſt made, and that even diſtillation cannot be abſolutely depended upon for a perfect ſeparation of it, I deſired Mr. Winch to prepare me a quantity of oil of vitriol in ſuch a manner, as that he could engage for its containing no ſpirit of nitre whatever, and with this I was determined to make my laſt experiment, and acquieſce in the reſult, whatever it ſhould be.

Accordingly Mr. Winch having furniſhed me with this oil of vitriol, I diſſolved in it 6 dws. of very clean iron, and diſtilling it to dryneſs, in a long necked retort, I received the common air a little phlogiſticated

cated, a little fixed air, much vitriolic acid air, and laſtly 18 ounce meaſures of dephlogiſticated air. The iron that remained undiſſolved weighed 23 grs. ſo that the air was yielded by 5 dwts. 1 gr. of iron. The nitre weighed 7 dwts. 13 grs. ſo that there probably remained a quantity of oil of vitriol in the nitre, and conſequently, had the heat been greater, more air might have been procured.

To try what might be done with a gun barrel, which could bear more heat than the glaſs retort, I put the reſiduum of the above-mentioned experiment, and alſo of that in which I had uſed 6 dwts. 4 grs. of iron together; and after they had been expoſed to the common air all night, I put them into the gun barrel. But, with as much heat as I could give to it in a charcoal fire, with a pair of bellows, I only got from it about an ounce meaſure of air, half of which was fixed air, and the reſt phlogiſticated. The ochre from the gun-barrel was black. I ſuſpect, however, that, could I have given theſe materials the ſame degree of of heat in an earthen retort, the air would have been both purer, and more in quantity.

Being now sufficiently satisfied that pure oil of vitriol would always yield dephlogisticated air with iron, it only remained to try whether the ochre remaining from the former experiment, from which air had been procured, would yield more air with more oil of vitriol, which is the case with red lead and spirit of nitre.

Accordingly, I put more oil of vitriol to this residuum (observing that it became very hot by this mixture, as red lead does with spirit of nitre) and then, with a red heat, in a glass retort, it yielded a quantity of vitriolic acid air, no fixed air, but 24 ounce measures of dephlogisticated air; when, the retort being melted, a good deal of the air was necessarily lost; for the produce of air had not begun to slacken when this accident happened, and removing the retort from the fire, I found only about half of the matter turned red, while the remainder was white. From this circumstance I concluded, that before I had not got more than half the air that it would have yielded. Reſuming the proceſs in a gun-barrel, I actually got about as much air as I had done before.

I had not now the leaſt doubt remaining but that the acid of vitriol, at leaſt with iron, is capable of properly *generating* dephlogiſticated air, as well as the acid of nitre with lead, or any other ſubſtance whatever. All this trouble I was led to take in conſequence of entertaining an unreaſonable doubt with reſpect to the experiment made with the Roman vitriol, of which an account is publiſhed in my ſecond volume; and, indeed, for want of reflecting properly on that made with *alum*, of which an account may be ſeen in my firſt volume.

To complete my experiments on the vitriolic acid and iron, I took half an ounce of the common ruſt of iron, ſuch as is uſed by apothecaries; and pouring upon it a quantity of that acid, obſerved that it imbibed it very eagerly, and became of a dark and almoſt a black colour. Then uſing a gun-barrel, I got from it two or three pints of of air, all of which was fixed air, but with a large residuum, about a third of the whole, phlogisticated air.

As the common rust of iron contains a good deal of phlogiston, I did not expect any better result from this experiment. But having, in some measure, purified it by this process, I put more oil of vitriol to what remained of the rust of iron, and then I got from it only a little fixed air, and sixteen ounce measures of dephlogisticated air.

It is evident both from these experiments with the vitriolic acid, and those cited in my second volume with spirit of nitre, that the earth of iron is easily converted into air; provided (which I think the most probable) that any earth enters into the composition of air. Should it be of this kind of earth that the bulk of atmospherical air in fact consists, it may perhaps help to account for the magnetism of the whole globe of the earth. This hint was suggested to me by Mr. Michell.

SECTION XXII.

Of the Production of Deplogiſticated Air by Means of the Vitriolic Acid, from other Metals.

HAVING got an indiſputable production of pure air from *iron*, by means of the vitriolic acid, it was natural for me to proceed to ſimilar experiments on *other metals*, with the ſame acid. And, in the firſt place, I made the proper trials with the two remaining kinds of vitriol, the *blue*, into which copper enters, and the *white*, which is compoſed of zinc; and having now no doubt remaining with reſpect to the purity of the vitriolic acid which enters into the compoſition of theſe kinds of vitriol, I contented myſelf with ſpecimens bought at the ſhops, and did not think it neceſſary to take the trouble to compoſe them myſelf.

In my firſt trial with an ounce of *blue vitriol* I got no air at all, neither vitriolic acid

acid air, fixed air, nor dephlogisticated air. This want of succefs, I imagine, was owing to my not being able to apply sufficient heat, in the manner in which I then made the experiment. For I succeeded better another time, when from about half an ounce of blue vitriol, in a glafs veffel, I got a little fixed air, and one ounce meafure of dephlogifticated air. The veffel breaking, I put the materials into a gun-barrel, and then got from them about 25 ounce meafures of dephlogifticated air, with hardly any more fixed air. The greateft part of this air was very turbid.

In the next place, I diffolved copper in oil of vitriol; and having put half an ounce of copper to a quantity of oil of vitriol, in a glafs retort, and diftilled it to drynefs, I got, befides vitriolic acid air, a quantity of fixed air, and an ounce meafure of dephlogifticated air; when the glafs was melted, and fome air efcaped. Breaking the hard mafs within the retort, when it was cold, the outfide was of a brownifh colour, inclining to yellow, and the inſide white.

Taking theſe materials from the retort, I put them into a gun-barrel; and, with as much heat as I could apply in a charcoal fire, with a pair of bellows, I got from them beſides fixed air, of which there might be an ounce meaſure in all, ten ounce meaſures of dephlogiſticated air. I found that not more than half the copper was diſſolved: for though there was vitriolic acid enough for the purpoſe, yet the pieces of copper not being very thin, a cruſt had been formed on the outſide of them, that defended them from the farther action of the acid, even in a boiling heat; ſo that I concluded that, had the copper been completely diſſolved, and the proceſs managed in the beſt manner about 30 ounce meaſures of dephlogiſticated air might have been procured.

To finiſh my experiments on the three vitriols, I took an ounce of calcined *white vitriol*, and, with a gun-barrel, I got from it it a great quantity of vitriolic acid air, ſome fixed air, with five ounce meaſures of dephlogiſticated air. At another time, from one ounce of this kind of vitriol, but uncalcined, I got only about two ounce meaſures of air, part of which was fixed air, and part dephlogiſticated air; not reckoning a great quantity of vitriolic acid air, which came, as uſual, before the dephlogiſticated air.

To proceed with *zinc*, as I had done with the iron and copper, I put a quantity of oil of vitriol to half an ounce of *flowers of zinc*; and, in a gun-barrel, got from it three ounce meaſures of air, a ſmall part of which was fixed air, and the reſt nearly as good as common air. Had I made uſe of a glaſs veſſel, I make no doubt but that I ſhould have got much more air, and much purer. For whatever it be in a proceſs that injures air, it leſſens the quantity of it. Three or four times the quantity of pure, or dephlogiſticated air, muſt be uſed to make a given quantity of common air, and ſtill more is requiſite to make the ſame quantity of phlogiſticated air.

I did not think it of much conſequence to my purpoſe to go through all the metals with this proceſs, and therefore only made a trial of ſuch as I happened to have at hand.

With *ſilver* I had no ſucceſs; owing, perhaps, to its requiring more heat than I could apply in a glaſs veſſel. I made the ſolution in a flint glaſs retort, and this happening to break when it was evaporating to dryneſs, I removed the maſs, which was yellow, into a green glaſs retort; and, melting it, I got from it about an ounce meaſure of fixed air, which might poſſibly come from its being a little time expoſed to the common air, in transferring it from one retort to the other. But after this I got no more air of any kind, though the maſs continued liquid, and even red hot at the bottom of the retort a conſiderable time; and at length the retort, unable to ſuſtain any more heat, melted.

Turbith mineral, which is made by a ſolution of quickſilver in oil of vitriol was

one

one of the ſubſtances from which Mr. Landriani procured dephlogiſticated air; though, for the reaſon mentioned above, I had not profited by his obſervation. But mercury being always at hand for the purpoſe of my experiments, I made trial of it, as of other metals in this courſe; and though I did not aſcertain the exact quantity of dephlogiſticated air that may be procured from a given quantity of mercury by this means, I, however, fully ſatisfied myſelf, that a very great quantity may be procured from it, and the proceſs itſelf is a peculiarly pleaſing one.

I diſſolved an ounce of quickſilver, purified by agitation in water, in pure vitriolic acid, in a green glaſs retort. During the diſtillation to dryneſs the retort broke; but collecting the materials as well as I could (in which perhaps one-tenth of the whole might be loſt) I put them into a freſh retort, and, expoſing them to a red heat, got from them a great quantity of vitriolic acid air, a good deal of fixed air, and about fifty ounce meaſures of dephlogiſticated air.

During the proceſs the ſolution boiled violently in the form of a red liquor, while the upper part of the retort was coated with a whitiſh ſort of matter. As the heat reached this coating, it alſo became red; and during the whole proceſs that which evaporated was collected on the ſides of the retort, and then deſceded to the bottom, like drops of blood, or red ink, ſo as to make a very pleaſing appearance. After the proceſs, a very little reddiſh matter remained at the bottom and on the ſides of the retort, which, as well as that which was collected at the neck of the retort, became white when it was cold. Very little of the quickſilver was revivified.

That I might form the better judgment of the quantity of air that might be extracted from an ounce of quickſilver, I collected, as well as I could, all the matter that adhered to the neck of the retort, and expoſing it to the heat a ſecond time, I got ten ounce meaſures of air more, with the ſame phenomena as before. Still, however, much of the matter adhered to the neck of the

the retort; so that how much air might have been procured, if the most had been made of the solution, I cannot tell with exactness.

As the breaking of the retort in the middle of the former process (in consequence of which the materials were exposed to the common air, and cooled in it) might leave some suspicion that the dephlogisticated air procured had been imbibed from the atmosphere, I repeated the process with a view to that circumstance. Dissolving an ounce of pure mercury in two ounces of pure oil of vitriol (frequently distilled) in a small retort with a long neck, the end of which was always immersed in quicksilver, or water; after the vitriolic acid air came over, which made lime water turbid (owing probably to a mixture of fixed air) I received twenty ounce measures of dephlogisticated air; when, the retort melting with the heat put an end to the process. My purpose, however, was sufficiently answered, as I had fully ascertained the production of dephlogisticated, if not of fixed air also, from

from these materials, without the help of any thing that might have been communicated to them from the atmosphere. When I come to treat of fixed air, I shall produce sufficient proof of the generation of fixed air from the acid of vitriol; as in my former publication, I shewed that it was sometimes indisputably generated from spirit of nitre; so that I then concluded that it was a modification of that acid.

It is remarkable that, either by means of oil of vitriol, or spirit of nitre, quicksilver yields a very great quantity of dephlogisticated air; but with this difference, that in the process with spirit of nitre, almost the whole of it (that is, if the process be conducted with care, with the loss of not more than the twentieth part of the mercury) is revivified, and therefore may be used again and again; whereas, in the process with the oil of vitriol almost all the mercury is lost.

The only metalic substance that I could conveniently make a trial of after this was tin; but the process being made in a gun-barrel,

barrel, it yielded no dephlogisticated air at all. For this purpose I moistened an ounce of *putty* (which I was assured was not of the common sort, but a pure calx of tin) with oil of vitriol, and I got a little fixed air, and two or three ounce measures of phlogisticated air. It is very probable that the phlogiston might come from the gun-barrel, and, by injuring the air, might make it yield a small quantity of phlogisticated, instead of a large quantity of dephlogisticated air. What this process would have yielded in a glass vessel I cannot tell.

SECTION XXIII.

Of the Production of Dephlogiſticated Air from EARTHY SUBSTANCES *by Means of the Vitriolic Acid.*

MY obſervations on the ſubject of this ſection have not been many, but they are ſufficient to ſatisfy me that pure air may be procured by the acid of vitriol from *earthy ſubſtances* that are not of a metalic nature; though, as was the caſe with the acid of nitre, not, in general, in ſo great abundance as from the metalic earths. But what might be the reſult of trials on a greater number of earthy ſubſtances I cannot pretend to ſay.

One of the firſt ſubſtances from which I extracted pure air, as I obſerved before, was *alum*, that is the earth of alum united to the acid of vitriol. But having overlooked that experiment, and not having got any good air from alum in my proceſs with a burning lens in mercury (though indeed

indeed the quantity was too ſmall for the purpoſe) it did not occur to me to make any farther trial of it, till I was engaged in the preſent courſe of experiments. I was now, however, fully ſatisfied, that dephlogiſticated air may be procured from it, though probably in no great quantity.

When I had well calcined a quantity of alum, I put it into a glaſs veſſel, and with a red heat I got from it a little fixed air, and ſome that was clearly dephlogiſticated; but an accident interrupting the experiment, I could not judge of the quantity that might have been procured. At another time, I got a pretty large quantity of air from calcined alum, all that it could be made to yield in a common fire, urged with a pair of bellows. The bulk of it was phlogiſticated air, with about half fixed air, the laſt produce not being quite ſo good as common air, though it was nearly ſo. Part of the alum had a tinge of black, acquired from the ſmoke of the fire in which the calcination was made; and this circumſtance might contribute to deprave the air.

 Laſtly,

Laftly, from an ounce of calcined alum, prepared fome months before, I got about fix ounce meafures of air, all quite as good, or better than common air, and without any fixed air in it. The procefs was in a gun-barrel, and the refiduum of the alum was very hard. This I moiftened with oil of vitriol, ftill keeping it hard and dry; and, in a gun-barrel, it yielded again two or three ounce meafures of air, chiefly fixed air, and at laft fome that was about as good as common air. After this it was remarkable that this matter abforbed air, perhaps about an ounce meafure in all. This I obferved twice, and it may be worth while to inveftigate this circumftance a little farther.

To half an ounce of *quick lime*, I put oil of vitriol till it weighed 1 oz. 4 dwts. when it made a hard mafs. This I pounded, and putting it into a gun-barrel; I got from it, in all, about ten ounce meafures of air, the greateft part of which was fixed air; but towards the laft, when the heat was as great as I could make it, in a

common

common fire, urged with a pair of bellows, the residuum was as good as common air, or rather better. This air came over very turbid.

Manganese yielding dephlogisticated air without the help of any acid, it might be thought more proper for the production of air with that assistance, as *minium* is with respect to the nitrous acid. I therefore tried it on the 15th of April, when to one ounce of this substance, which had been kept red hot a long time on the 10th of November preceeding, I put some oil of vitriol, which it imbibed eagerly; and then got from it about twelve ounce measures of air, the whole of which was fixed air, except about one ounce measure, which was about as good as common air. In this experiment I believe I made use of a gun barrel, so that probably more, and better air would have been procured in a glass vessel.

SECTION XXIV.

Attempts to procure Air from various Subſtances by means of Spirit of Salt.

MR. Landriani had informed me that he had got dephlogiſticated air from *corroſive ſublimate*, as well as from *turbith mineral*. But trying this, in the beſt manner I could, immediately upon the receipt of his letter, I was not able to procure any air from it; and though I have varied the proceſs, I am ſtill unable to procure any. It was this failure (from what cauſe I cannot tell) that prevented my proceeding to the turbith mineral at that time, as has been mentioned before.

I firſt put a quantity of corroſive ſublimate into a tall glaſs veſſel, then filling it up with quickſilver, I inverted it in a baſon of the ſame, and expoſed the ſublimate to as much heat as the glaſs would bear, in the manner deſcribed in the Introduction. The glaſs was even melted; but when all

was

was cool, it appeared that no air had been produced. The mercury roſe and filled all the interſtices of the ſublimate. I then put two ounces of this ſubſtance into a green glaſs retort, which will bear a greater degree of heat than flint glaſs; and by degrees covered it with live coals, but all that followed was the ſublimation of the matter into the neck of the retort, and no production of air. Even the common air, that came over firſt, was not at all altered.

I was not able to make much more of *common ſalt*. From an ounce of it, in as ſtrong a red heat as I could give it in a glaſs retort with a long neck, and in ſand (in which it may be made to bear more heat than when ſurrounded with live coals) I got about two ounce meaſures of air, the firſt part of which was fixed air, and the laſt phlogiſticated air, extinguiſhing a candle, and not affected by nitrous air. I have, however, at various times, repeated this experiment, and once with the heat of a ſmiths fire; but getting little or no air, I rather ſuſpect that the phlogiſticated air in the preceeding experiment came from ſome particles of foreign matter, that, unperceived by me, might be mixed with the ſalt, rather than from the ſalt itſelf.

As *iron* is eaſily ſoluble in ſpirit of ſalt, and yields abundance of inflammable air, I was in hopes that this ſolution, diſtilled to dryneſs, might yield dephlogiſticated, or ſome other kind of air; and with this view I diſſolved half an ounce of iron in ſpirit of ſalt, and diſtilled it to dryneſs in a green glaſs retort. But I got only a very ſmall quantity of fixed air, juſt ſufficient to precipitate lime in a veſſel of lime water, in which the air was immediately received, and yet the whole maſs was kept perfectly fluid with heat.

In diſſolving this iron in ſpirit of ſalt, I obſerved that when the large bubbles burſt, they were full of a whitiſh matter, reſembling the cloudy appearance of nitrous air when it is produced very rapidly. It ſeems, therefore, that all the kinds of air, by whatever acids they are procured, contain earth, either in a diſſolved ſtate, or as a cona constituent principle in their composition. For when much heat is used in the production of any kind of air, it contains more earth than it can hold when it is reduced to the temperature of the atmosphere.

To make a final experiment of this kind upon iron, which yields air with peculiar readiness in most other processes, I dissolved 3 dw. 8 gr. in distilled water impregnated with marine acid air; but, distilling it to dryness in a long necked retort, and applying as much heat as the glass would bear, I got nothing but a very small quantity of fixed air, the residuum of which was phlogisticated.

The last trial I made of this acid was with *quick lime*, which is dissolved with great rapidity, and in great abundance by spirit of salt. Half an ounce of it I saturated with spirit of salt, and putting it into a green glass retort, I got no air at all from it, even in red heat; but the last portion of the common air that came over was phlogisticated. I then put it into a gun barrel, and with as much heat as I could give it in a common fire, urged with a pair of bellows, I got from it about twenty five ounce meaſures of air, part of which was fixed air, and the reſt inflammable, burning with a blue flame. This, I have little doubt, came from the iron; and the mixture of fixed air from the lime would make it burn blue. When the production of this air was pretty quick, it was turbid, as in other caſes.

Upon the whole, I think I may conclude from the experiments recited in this ſection, that the marine acid differs eſſentially from both the vitriolic and nitrous in this, that it cannot, by any combination whatever, be made to yield dephlogiſticated air, at leaſt with the degree of heat that I was able to apply.

SECTION XXV.

Miscellaneous Experiments relating to Dephogisticated Air.

1. *The very great diminution of Dephlogisticated Air by Nitrous Air.*

IT appears from my first observations on the properties of deplogisticated air, that, in general, when two equal measures of nitrous air are mixed with one measure of it, the whole is reduced to half a measure, and sometimes, when I thought it peculiarly pure, to one sixth of a measure. I have since, on one particular occasion, produced this kind of air in a state of so much greater purity as appeared very extraordinary to myself, and I doubt not will be thought so by others.

Having, for a purpose that will be mentioned in the account of my observations on fixed air, kept a solution of mercury in spirit of nitre for several months, in a phial with a ground stopper, I put it into a retort with a long neck, and, in a sand

heat,

heat, received in the firſt place, the nitrous air it yielded, and then without removing the retort from the fire, the dephlogiſticated air. Uſing both the nitrous and dephlogiſticated air of the ſame produce, I obſerved that two meaſures of the former and one of the latter mixed together, occupied, after the efferveſcence was over, the ſpace of no more than three hundred parts of a meaſure.

It was impoſſible for me to be miſtaken with reſpect to this remarkable fact; for the tube in which I meaſured the reſiduum was ſo long, in proportion to the capacity of the phial which I uſed as a meaſure, that a hundredth part of a meaſure exceeded the eighth of an inch. Repeating the experiment, I found that two meaſures of nitrous air were rather more than ſufficient to ſaturate one meaſure of the dephlogiſticated air; ſo that, poſſibly, had the former experiment been made with more circumſpection, the diminution, extraordinary as it was, would have been ſomewhat greater. Indeed it cannot be ſuppoſed, that

exactly

exactly two meaſures of nitrous air ſhould be the preciſe quantity that would produce the greateſt diminution. It ſhould alſo be conſidered, that a ſmall portion of air might be yielded by the water in which the experiment was made.

Upon the whole, therefore, I am inclined to think that, were it poſſible to make both the nitrous and dephlogiſticated air in the greateſt purity, and then to mix them in ſome exact proportion, the aerial form of them both would be entirely deſtroyed, the whole quantity ſeeming to diſappear, as in the mixture of alkaline and acid air. But whereas a white ſaline ſubſtance is the immediate viſible reſult of this mixture, there is no viſible produce from the other, the whole, whatever it be, being diſſolved in the water; ſo that, this would probably be the more ſtriking phenomenon of the two; and the mixture of acid and alkaline air never fails to excite a good deal of aſtoniſhment, eſpecially when they are previouſly made, and contained in ſeparate veſſels, and then ſuddenly mixed together, by transferring them from one veſſel to another in a trough of quickſilver.

Willing to get dephlogiſticated air in a ſtate of the greateſt purity, and having obſerved that it ſometimes comes over mixed with the *red vapour* of ſpirit of nitre, ſome times quite tranſparent, and again exceedingly turbid with the white matter, depoſited in the cold recipient; I thought that, poſſibly, it might differ in purity according as it was procured in theſe different circumſtances. To try this, I diſſolved a quantity of mercury in ſpirit of nitre, and putting the ſolution into a long necked retort, I diſtilled the whole to dryneſs; then, placing the retort in a ſand heat, I received all the air that came from it in ſeveral portions, firſt that which was mixed with the red vapour, then that which came while the tube was quite tranſparent, then that which was very cloudy with the whitiſh matter, and laſtly that which came after it was tranſparent again; but I did not find that there was any ſenſible difference between any of theſe portions of dephlo-

dephlogiſticated air. They were all equally pure. The red vapour certainly tends to injure the air, but I ſuppoſe more time was requiſite to produce a ſenſible effect than this proceſs admitted.

2. *Of procuring dephlogiſticated Air by means of crude Nitre.*

It is much to be wiſhed, that ſome method could be found of making dephlogiſticated air in great quantities, and very cheap; and I am not without hopes, that, in time, much cheaper proceſſes will be hit upon than thoſe that are now in uſe for that purpoſe. At the time of my laſt publication I generally made uſe of *ſpirit of nitre*, which is a dear article in chemiſtry. At the ſame time I had alſo procured air from nitre itſelf, though only in an inconſiderable quantity. Mr. Scheele, however, I find, generally makes uſe of *nitre only* for whatever quantity of this kind of air he makes uſe of; and I had been informed that ſome perſons had procured great quantities of this air from a mixture of *ſand and nitre.*

nitre. This I imagined to be occaſioned by the acid of the nitre being gradually diſengaged by the heat, and uniting with ſuch earthy matter as was at hand to combine with it in this new manner. More air might be procured by this means, becauſe, when the ſpirit of nitre previouſly formed is made uſe of, far the greater part of it is thrown off by the heat of the proceſs, and never contributes to the formation of air at all.

I therefore made a few trials of a mixture of nitre and various kinds of earth, and found that, in ſeveral caſes, more of the air would be procured by means of crude nitre than by the nitrous acid; ſo that a conſiderable ſaving would, no doubt, be made by this means. But then I found that much more *heat* was neceſſary for the purpoſe, ſo that the expence of fuel would be more conſiderable. I am ſatisfied that it is by means of much greater heat than I have ever applied that Mr. Scheele gets ſo large a quantity of dephlogiſticated air from nitre only; and the celebrated Mr. Pott of

Berlin, I am informed, has expelled all the acid of nitre by mere heat, leaving nothing but its alkaline base. Had the elastic matter which he expelled been collected, it would, no doubt, have been dephlogisticated air; and it would be curious to ascertain the quantity of this air from a given weight of nitre. As to myself, I have never had the use of a regular laboratory, and hitherto have never applied more heat than I could raise in a common fire, urged with a pair of bellows; except that, on particular occasions, I have had recourse to a smith's fire.

In company with Mr. Magellan, I endeavoured to procure dephlogisticated air from nitre and common sand; but for want, I suppose, of sufficient heat, the quantity we got was inconsiderable; and he has since informed me, that the sand is not at all necessary, but only a greater degree of heat to be applied to the nitre. I found, however, that, with the same degree of heat, I could get more air from a mixture of nitre and various other substances, than I could

from

from the nitre alone; though I got more air by means of nitre than of ſpirit of nitre, as I obſerved above. The experiments I made were as follows,

From leſs than half an ounce meaſure of pounded ſalt petre, and the ſame quantity of *ſalt of tartar*, well mixed together, I got, in a glaſs veſſel, with a red heat, 17 ounce meaſures of dephlogiſticated air, beſides about a fourth part of fixed air mixed with it, in all the ſtages of the proceſs; whereas from a whole ounce meaſure of ſalt petre, treated in the ſame manner, without any mixture of ſalt of tartar, I got only 13 ounce meaſures of dephlogiſticated air, beſides a ſmall quantity of ſuch air as made lime water a little turbid.

To try the difference between nitre and ſpirit of nitre, I made uſe of the *flowers of zinc*. Half an ounce of theſe mixed with a quarter of an ounce of ſalt petre, in a glaſs veſſel, and a red ſand heat, yielded 22 ounce meaſures of dephlogiſticated air; but the whole proceſs took up no leſs than three hours. The air often came very irregu-

irregularly, though ſometimes pretty equably. The remainder of the materials weighed conſiderably leſs than the flowers of zinc. From the ſame quantity of the flowers of zinc and a quarter of an ounce of ſtrong ſpirit of nitre, I got not more than 11 ounce meaſures of dephlogiſticated air, or half the quantity that I got before; the tubes through which it was conveyed being filled with red fumes, by which much of the ſpirit of nitre muſt neceſſarily have been loſt.

3. *Of the ruſting of Metals in Air.*

It is generally thought, I believe, that metals expoſed to the open air are corroded, and contract ruſt, by means of ſome *acid vapour* contained in it. I thought it poſſible, however, that very pure air might have ſuch an affinity with phlogiſton, as to deprive ſome metals of it, without the aid of any acid. To try this, I filled an eight ounce phial with very dry clean nails, and then with quickſilver, which I diſplaced by very pure and dry dephlogiſticated air, and

and left it inverted in a baſon of quickſilver on the 13th of April 1778. At this time, *viz.* the 26th of January 1779, I find that one tenth of the whole quantity of air is gone, the quickſilver having riſen ſo high in the phial. I therefore take it for granted, though I cannot perceive any ruſt on the nails, that my conjecture is well founded; that the air has been diminiſhed by means of phlogiſton from the iron, and that in *time*, if the quantity ſhould be ſufficient, the ruſt will be apparent.

4. *Of the Detonation of Nitre.*

The diſcovery of dephlogiſticated air throws great light on many very important facts in chemiſtry, but upon none more than upon that very difficult and ſtriking one of the *detonation of nitre*, concerning which the moſt improbable conjectures have been advanced by the moſt eminent philoſophers and chemiſts. This detonation is the ſudden inflammation produced by the contact of various ſubſtances containing phlogiſton and nitre, when either of of them is red hot. The hypothesis that has been thought the most satisfactory is that of Mr. Macquer, who supposes that, in these circumstances, an union is formed between the pure nitrous acid and phlogiston, similar to that which is formed between the vitriolic acid and phlogiston in the composition of sulphur. He therefore supposes that, in this case, *a nitrous sulphur* is formed, and that this substance is of so inflammable a nature, that it cannot exist a moment without actual ignition.

But I would observe that, supposing this hypothetical nitrous sulphur to be actually formed, yet if it resemble other combustible substances, the vitriolic sulphur for instance, or any other whatever, in a property that is common to them all without exception, it cannot be inflamed but in contact with air; which, according to conclusions clearly drawn from my experiments, and all other observations, is saturated with phlogiston by the process, and when saturated can take no more, let the substance

ſtance that is heated in it be ever ſo combuſtible; and conſequently, in thoſe circumſtances, all inflammation muſt be impoſſible. Whereas Mr. Macquer acknowledges, that this nitrous ſulphur is capable of the moſt violent inflammation in the cloſeſt veſſels, where there is no acceſs of air, and it is well known that compoſitions of gunpowder are made to burn even under water.

Now the doctrine of dephlogiſticated air ſupplies the eaſieſt ſolution imaginable of this very difficult phenomenon. For it appears that the nitrous acid cannot be heated to a certain degree, in contact with any earthy matter, without producing dephlogiſticated air; by the help of which all combuſtible ſubſtances burn with the greateſt violence, much more than they can be made to burn with in common air. Here then I ſuppoſe that the moment the acid of nitre, contained in the nitre, and the earth of the coal, for example, thrown into it become red hot, in contact with each other, dephlogiſticated air is produced; and in this air the remainder of the charcoal, being

likewiſe

likewiſe red hot, burns with the violence that is obſervable in the experiment; while, at the ſame time, other portions of the nitrous acid are forming, with other parts of the ſame decompoſed charcoal, the union that conſtitutes more dephlogiſticated air; and thus the detonation continues, till all the charcoal, or all the nitre, is conſumed; the acid not being *loſt*, as ſome chemiſts expreſs it, but entering into the compoſition either of the dephlogiſticated air, or of ſome other kind of air, that may be generated in the proceſs.

Let any perſon but attend to the phenomena of the detonation of charcoal in nitre, and that of the dipping a piece of hot charcoal into a jar of dephlogiſticated air, and I think it will be impoſſible for him not to conclude that the appearances are the very ſame, and muſt have the ſame cauſe. There is the ſame intenſe incandeſcence, an de ſame rapid conſumption of the charcoal in both caſes; and this is evidently owing to the eagerneſs, as I may ſay, with which this ſpecies of air, the moſt free from phlogiſton itſelf, ſeizes upon the phlogiſton of other bodies, in a ſufficient degree of heat. Such appearances cannot be produced in common air, which, being more than half ſaturated with phlogiſton already, can take but little more; and therefore, to produce an appearance any thing reſembling them, we are obliged to ſupply the fire with a current of freſh air thrown into it by bellows. But ſupplying a fire in the ſame manner with a current of dephlogiſticated air, which I have ſometimes done, has a moſt aſtoniſhing effect of the ſame kind, as I have obſerved in my former publications on this ſubject.

This method of explaining the detonation of nitre had occurred to me at the time of my firſt publication on the ſubject, and a ſhort hint of it, with a view to what becomes of the acid of nitre, will be found in my 2d volume, p. 60; but I thought it might be uſeful to give a more general account of it here. Many other important phenomena in chemiſtry will, I doubt not, admit of the greateſt illuſtration from this diſcovery; but my acquaintance with che-

 miſtry

miſtry being very partial, ſuch illuſtrations are not ſo likely to occur to me as they are to many other perſons.

As to the *nitrous ſulphur* of Mr. Macquer, I know of nothing more nearly approaching to it than *nitrous air*, which conſiſts chiefly, if not wholly, of pure nitrous acid, and phlogiſton, without any water. This, at leaſt, is ſimilar to the compoſition of vitriolic acid air, which a continued heat in a confined ſtate changes into ſolid ſulphur.

I ſhall conclude this article of deplogiſticated air and detonation, with an account of a very ſtriking experiment that I made with Mr. Bewly's pyrophorus, the receipt for which will be found in my third volume, p. 402, and which, I make no doubt, may be made with any good pyrophorus. I put a quantity of it into one of the ſmall jars which I uſe for experiments on air in quickſilver; then, filling up the veſſel with quickſilver, I inverted it in a baſon of the ſame, and threw up dephlogiſticated air at different times. It always occaſioned a ſudden and vehement accenſion, like the flaſhing of gunpowder, and the air was greatly diminiſhed, as might have been foreſeen.

SECTION XXVI.

Of the Preſence of EARTH *in atmoſpherical Air, or in dephlogiſticated Air, as the proper Origin and Baſis of it.*

HAVING never failed to get dephlogiſticated air from earth and ſpirit of nitre, and none at all from pure ſpirit of nitre itſelf, I concluded that dephlogiſticated air, and conſequently atmoſpherical air, which is only dephlogiſticated air in a ſtate of depravation, conſiſts of earth and ſpirit of nitre. The acid, I ſince conclude, is not the acid of nitre *as ſuch*, but an acid principle common to it and the vitriolic acid, or an acid of which thoſe two mineral acids are only different modifications. The Abbé Fontana and Mr. Lavoiſier, however, deny the preſence of earth in dephlogiſticated air, from having revivified, as they ſay, *the whole*

whole of a quantity of mercury diſſolved in ſpirit of nitre, after it had yielded a great quantity of both nitrous and dephlogiſticated air. Could this reſult be depended upon, it would certainly follow, that there could be no earth either in nitrous, or in dephlogiſticated air.

The account of their experiments I did not receive till my third volume on the ſubject of air was printed off. I had time, however, to repeat the experiments with ſome attention, and to give an account of the reſult of them in the preface to that volume. At that time I had found a clear loſs of 1$\frac{1}{4}$ dwt. from 17 dwt. 13 gr. of pure mercury, and I therefore concluded that ſo much of the calx of the mercury entered into the compoſition of the nitrous or dephlogiſticated air. I have ſince had leiſure to make this experiment with more attention than I was able to give to it before; and of the many that I made with this view, I ſhall recite the particulars of two, becauſe ſeveral things occurred in them that may be worth notice, though the general refult was nearly the fame with that of which a report has been made already.

I diffolved 17 dwt. 13 gr. of pure mercury, furnifhed me by Mr. Woulfe, in an equal weight of ftrong fpirit of nitre, and diftilled it to drynefs in a glafs retort with a long neck, bent fo as to be immerfed in water; the folution having been made in the fame retort, without ever being taken out of it. Then, giving it a very ftrong heat in fand, all the mercury that was revivified came over; and being carefully collected, there appeared to have been a lofs of 1$\frac{1}{2}$ dwt. very nearly. Making every allowance, I believe there was a clear lofs of 1 dwt. 6 gr.

In thefe experiments there are, however, *four* caufes of inaccuracy; the firft arifing from the quantity of folid matter that comes over diffolved in the liquid that is procured during the firft production of nitrous air; the fecond from the liquid that is diftilled in bringing the whole to a folid mafs; the third in the folid matter that fublimes ſublimes in the neck of the retort during the revivification of the mercury; and the fourth in the white matter that clouds the air, eſpecially when it is produced with rapidity. All theſe cauſes of error I attended to ſeparately, and found only the firſt and third to be at all conſiderable. After having found by experiment the amount of the loſs in all theſe caſes, I ſtill found a conſiderable deficiency in the weight of mercury after the experiment; and therefore ſtill conclude, that there is ſome earth in the air; but I do not ſay whether this earth be eſſential in its conſtitution, though I ſuſpect it to be ſo, or only *diſſolved* in it, and foreign to it, like water in air.

I diſſolved in ſpirit of nitre 18 dwt. 19 gr. of quickſilver which had been diſſolved and revivified again many times in former experiments, ſo that its purity may certainly be depended upon, and catching the liquor that diſtilled over all the time that the nitrous air was produced, I found that when it was evaporated, cryſtalized, and dried again, it weighed 3 dw. 15 gr. Putting this into a tall glaſs phial, and expoſing it to a red ſand heat, part of it was ſublimed, coating the glaſs in circular ſpaces with a coloured matter, in the following order, from the bottom, *yellow, red, yellow, green, whitiſh.* The part which was not ſublimed was of a beautiful light red, and weighed 2 dw. 12 gr. Scraping off the part that had ſublimed, and eſpecially the green, which was moſt copious, the whole, when mixed together, appeared in the form of a dirty brown, or yellowiſh matter, like Scotch ſnuff, weighing 12 gr. By trituration it yielded a good deal of quickſilver.

Taking all the precipitate, and mixing with it that which had been collected from the liquor that had diſtilled over during the ſolution, as mentioned above, and putting it into a retort, I expoſed it to as much heat as the glaſs would bear in a naked fire, and continued the diſtillation till nothing but a whitiſh ſtain was left at the bottom of the retort, and a very little yellowiſh matter

matter adhered closely to the sides of it, which could not be supposed to weigh more than a couple of grains. Collecting all the quicksilver, it weighed 17 dw. 18 gr; so that there had been a loss of one dwt.

In this manner of making the experiment, the quantity that sublimes is much less than usual. That the solid matter contained in the liquor that comes over during the distillation of the solution to dryness, after the production of nitrous air, is inconsiderable, and may be neglected, appeared from the following observation. I received in a cup all the liquor that came over in a process of this kind, observing that, at first, it was blue, but presently became colourless, by being exposed to the open air; but at last it was strong yellow spirit of nitre. This liquor exposed to a gentle heat intirely evaporated, having only an exceedingly slight yellow stain at the bottom of the earthen cup in which the evaporation was made.

As to the whitish matter that clouds the air, it is, when collected, so very inconsiderable with respect to weight, that it may be be very ſafely neglected. However in one of my proceſſes, all the particulars of which I do not think it worth while to recite, I carefully attended to this circumſtance, making the proceſs in ſuch a manner, that all the air, except a very little in the middle of the proceſs, came over without any turbid appearance whatever, and ſtill the reſult was nearly the ſame as that of the reſt.

In one of theſe proceſſes I obſerved that twice as much nitrous air was got from the ſolution of mercury after it was completely diſſolved, as during the ſolution, and the dephlogiſticated air was about three times as much as the nitrous.

It will appear from the above recited experiment, that conſidering the quantity of air, both nitrous and dephlogiſticated, procured by the ſolution and revivification of the mercury, and the ſmall loſs of mercury in the proceſs, that by far the greateſt part of the weight of air muſt come from the *acid* of which it conſiſts, the *earth* bearing but a ſmall proportion to it.

After

After the preceding experiments, I thought it might be poſſible to diſcover the earth which is in air, by decompoſing a quantity of dephlogiſticated and nitrous air in the ſame pure water, which muſt, of courſe, retain all that is ſolid in either of them. Accordingly I decompoſed thirty nine ounce meaſures of nitrous air, and nineteen of dephlogiſticated air, throwing out, at different times, ſix ounce meaſures of phlogiſticated air, in little more than two ounces of diſtilled water, which became a volatile ſpirit of nitre by the proceſs. There was no turbidneſs or any earthy matter viſible in it; but, evaporating it to dryneſs, there remained three or four grains of a red or dark brown earthy ſubſtance, part of which was inſtantly diſſolved in ſpirit of ſalt, and gave it a brown colour. Part of it I expoſed in the open air, from which it attracted moiſture. Poſſibly, however, the ſolid matter in this water might be ſo incorporated with it, as to be evaporated along with it; for I made it boil during the evaporation. Conſidering the quantity of of earthy matter that remains after the diſtillation of the pureſt water, the reſiduum in this experiment will be thought inconſiderable; and I own it did not anſwer my expectations.

With a view to prevent the liquor in which was the mixture of nitrous and dephlogiſticated air from becoming acid, I repeated this proceſs in cauſtic alkali (though I found afterwards that, by long keeping, it had imbibed a good deal of fixed air) when the appearances were pleaſing enough, and the reſult rather favourable to the ſuppoſition of the preſence of earth in air. Immediately on mixing theſe two kinds of air over this alkaline liquor, there was a beautiful precipitation of white vapour, and again when the ſaturation was nearly completed; but there was little or nothing of this appearance in the middle of the proceſs. Pouring the alkali, after this, into another veſſel, a denſe white vapour iſſued from it. All theſe appearances were more ſtriking after I had repeated the proceſs ſeveral times in the ſame alkali. After the

whole

whole procefs the liquor had acquired a yellowifh colour.

This experiment was made on the 19th of September 1777, and looking at the alkaline liquor on the 14th of December 1778, I obferved that a white matter was depofited from it; but whether this came from the air that was decompofed in it, or not, I do not pretend to fay.

SECTION XXVII.

Various Obfervations relating to the Diminution of common Air.

1. *Of the Purity of Air in different Circumftances.*

WHEN I firft difcovered the property of nitrous air as a teft of the wholefomenefs of common air, I flattered myfelf that it might be of confiderable practical ufe, and particularly that the air of diftant places and countries might be brought and examined together, with great eafe and fatisfaction; but I own that hitherto I have rather been difappointed in my my expectations from it. My own obſervations have not, indeed, been many; but according to them the difference of the open air in different places, as indicated by a mixture of nitrous air, is generally inconſiderable; and I have reaſon to think that when very unwholeſome air is conveyed to a great diſtance, and much time elapſes before it is tried, it approaches, by ſome means or other, to the ſtate of wholeſome air. At leaſt ſuch I have found to be the worſt air that has at any time been ſent to me in Wiltſhire from diſtant manufacturing towns and workſhops &c. in them, where the air was thought to be peculiarly unwholeſome. I am ſatisfied, however, from my own obſervations, that air may be very offenſive to the noſtrils, probably hurtful to the lungs, and perhaps alſo in conſequence of the preſence of phlogiſtic matter in it, without the phlogiſton being ſo far *incorporated with it*, as to be diſcoverable by the mixture of nitrous air.

I gave ſeveral of my friends the trouble to ſend me air from diſtant places, eſpecially, from

from manufacturing towns, and the worst they could find to be actually breathed by the manufacturers, such as is known to be exceedingly offensive to those who visit them; but when I examined those specimens of air in Wiltshire, the difference between them and the very best air in this county, which is esteemed to be very good, as also the difference between them and specimens of the best air in the counties in which those manufacturing towns are situated, was very trifling.

Mr. Boulton of Birmingham was so obliging as to send me a great variety of specimens of air from that manufacturing town, along with an account of his own examination of them by the test of nitrous air. I shall only note his account of four of the specimens, including the best and the worst, and reducing his numbers to my own.

	Meaſures.
The air in a garden near the new church	1.39
The bottom of the old church ſteps, very low and cloſe	
The middle of Mr. Taylor's manufactory	1.45
The Horn Button manufactory	

When I examined them myſelf, on the 12th of December 1777, the former was as nearly as poſſible the ſame with the air of pretty high ground in Wiltſhire; ſo that the difference between the worſt air in the manufactories at Birmingham and very good air was .06. On the 3d of July following, I examined the remainder of the ſame ſpecimens of air again, and found the difference between them and good air to be .02; and at the end of October it was only .01.

Dr. Percival alſo was ſo good as to ſend me ſeveral ſpecimens of air from Mancheſter, and one from his country houſe at Hart-hill, about three miles from Mancheſter, the higheſt and healthieſt ſituation in that part of the country. The air of this place was nearly the ſame with that of Wiltſhire; and when I examined the ſpecimens he ſent me on the 3d of July 1778, the meaſures of the teſt for this air were 1.27, of the air from a weaving ſhop in Mancheſter 1.305, and of the market place 1.295. The difference therefore between the former and pure air was only .035, and of the latter only .025.

The worſt air that I have yet found breathed by men, and that was ſent from a diſtance, was from a coal-pit in the neighbourhood of Briſtol. For the difference between good air and that which was taken in the ſhaft of the pit ten yards below the mouth was .07, and between the ſame and that which was taken where the men were at work was .21.

Mr. William Vaughan took the trouble to procure me a ſpecimen of air from a calico printing houſe, which was exceedingly offenſive, and I have no doubt of its having been taken very properly, and having been well ſecured from all communication with the external air; and yet when I examined it in Wiltſhire the difference between it and good common air was only .02.

Mr. S. Vaughan, ſenior, on his paſſage from Jamaica, brought me two bottles of air, one from the hold of the ſhip, intolerably offenſive, the other the freſh air above deck in about 30′ N; but the difference between theſe ſpecimens of air, and the air of Wiltſhire, was quite inconſiderable.

I have frequently taken the open air in the moſt expoſed places in this country at *different times of the year*, and in different ſtates of the *weather, &c.* but never found the difference ſo great, as the inaccuracy ariſing from the method of making the trial might eaſily amount to, or exceed.

2. *Of the State of the Air in* HOT-HOUSES.

There is generally a ſenſe of oppreſſion, or difficult reſpiration, felt on entering a *hot houſe*, which ſeems to proceed from ſomething different from mere heat; for we feel nothing of that ſenſation in an equally

equally warm, well aired room; but my obſervations on this kind of air would not have indicated any ſuch thing. On the 3d of June 1778, I took the air in three ſeveral hot houſes adjoining to each other, but having different degrees of heat, and found that one meaſure of that air and one of nitrous air occupied the ſpace of 1.29 meaſures; when the reſult of the ſame experiment with the external air, taken at ſame time, was 1.27, a difference certainly very inconſiderable.

3. *Of the Effect of the* PERSPIRATION *of the Body on Air.*

That *breathing* contaminates air is well known, and this makes a difference in air that is eaſily diſtinguiſhed by a mixture of nitrous air. Having obſerved this, I had the curioſity to try whether air was injured in the ſame manner by any effluvia attending the ſenſible, or inſenſible, *perſpiration* of the covered parts of the body; and, with reſpect to *myſelf*, I think I have given it a very fair trial, and can affert, that I never found air to be at all fenfibly injured in thofe circumftances, but rather, if I could depend upon my application of the teft of nitrous air for fo fmall a difference, it was fomething better than the external air. I have fat an hour with my arm in a trough of very warm water, and my warm hand in a glafs jar placed with its mouth in the water (my hand, of courfe, perfpiring, though infenfibly, all that time) but when I examined the air within the jar immediately afterwards, it appeared not to have been the leaft injured by the procefs.

But what I expected to produce a much more fenfible change in the air was the perfpiration under the arm pits, after walking, or ufing much exercife. For this purpofe, I have fometimes introduced phials of warm water, and poured it out, when I had introduced my hand as carefully as poffible into the place; but at other times I have put open phials, with perforations in the bottoms, and alfo open glafs tubes, three

three or four inches long, the orifices of which were ſuch as that I could eaſily cover them with my thumb or finger. This appearing to be the faireſt method of all, I made the greateſt uſe of it. For the air within the open tube muſt certainly, in the courſe of an hour or two, become of the ſame quality with the air on the outſide of it. In theſe trials alſo, I have preferred *walking* to any other kind of exerciſe, though I have tried ſeveral methods; becauſe, in walking, little or no motion is given to the air about the arm; and it is very eaſy to introduce one's hand, and, covering both the ends of the tube at the ſame time, to be quite ſure that the air within the tube is in that ſtate to which the perſpiration of the body had reduced it. But ſtill, after walking a long time, and making myſelf purpoſely as hot as poſſible, I have never found the air within the tubes in the leaſt degree worſe than the external air; but, as I have ſaid before, ſometimes ſeemingly a little better.

The experiment of this kind that I made with the moſt care was in pretty hot weather, on the 4th of June 1778. I put ſuch tubes as I have mentioned above under each of my arms, and after firſt working with a ſpade, and then walking about three miles, in which exerciſes I purpoſely made myſelf exceedingly hot, I withdrew the tubes with as much care as a good deal of experience had taught me, and I found that one meaſure of this air and one of nitrous air occupied the ſpace of 1.267 meaſures; when the ſame experiment being made with the beſt external air on the ſame day, the meaſures were 1.28. Every circumſtance in the application of the teſt was, as near as I could make it, the very ſame in both caſes.

4. *Of the State of the Air in* Dining-Rooms.

Large and *lofty rooms* are generally preferable to ſmall and low ones. But this is only the caſe when the ſame company confine themſelves in it the ſame ſpace of time,

time, with the doors, *&c.* ſhut; for, having more air to breathe, it will certainly require more time to contaminate it. But when the company is large, or proceſſes are going on that will effectually contaminate the air (as many candles burning in the room, hot victuals, continuing a long time upon the table, *&c.*) a ſmall room is much preferable, unleſs there be an opening in the top of the large room, that will eaſily promote a change of air in it. Becauſe the occaſional opening of the door in a ſmall room will generally produce a ſufficient change of a great part of the air; whereas the height of the door bearing but a ſmall proportion to the height of a large and well proportioned dining room, the opening of the door, or even its continuing open, has very little effect. The extreme offenſiveneſs of the air in theſe circumſtances is not perceived by perſons who ſit in it from the beginning, but it is immediately perceived by perſons who ſtep out of the room, and return to it.

Dining one time a in company of not more than eight or ten perſons, in a large and very lofty room; and being called out preſently after the cloth was removed, I was much ſtruck with the offenſiveneſs of the air on my return; and being willing to aſcertain the *degree* in which it was injured, I took occaſion, on ſome pretence or other, to pour the water from one full decanter into another, and putting in the ſtopper, ſaw that no body opened it till the company ſeparated. I then took the decanter into my laboratory, and examined the air at my leiſure; when it appeared to be much contaminated. For one meaſure of this air, and one of nitrous air occupied the ſpace of one 1.31 meaſures; when the ſame experiment being made with the air of a well ventulated room in the ſame houſe, the meaſures were 1.25. At the ſame time I breathed a quantity of air till it juſt extinguiſhed a candle, and found that the meaſures were 1.43. So that, had the air of the dining room received a little more than twice as much more phlogiſtic

giſtic matter, as it was charged with by the breathing of theſe eight or ten perſons, the effluvia of the victuals, *&c.* a candle would not have burned in the room. I would adviſe, therefore, that when ſuch large dining rooms are built, proviſion be made for letting out the vitiated air at the top of them. For breathing ſuch contaminated air ſo long a time as it is now the cuſtom to do, at and after dinner, muſt be very hurtful. Otherwiſe, if it were not inconvenient on other accounts, it would be better to have the dinner in one room, and the deſert in another.

5. *Of the Effect of* STEAM *on Air.*

Very early in the courſe of my obſervations concerning air, I found that the agitation of any kind of noxious air in *water* purified it to a certain degree, as alſo that the agitation of pure air in water depraved it ſo much, as to bring it to about the ſame ſtandard, *viz.* that in which a candle juſt goes out. It might, therefore, be thought, that *ſteam*, or the vapour of water, intimately

mately diffused through a quantity of noxious air, would much sooner imbibe the phlogiston with which it was charged; and several persons, particularly Mr. Keir, have even thought that the melioration of air by vegetation may be owing to the exhalation of moisture from plants in a vegetating state. I was very willing to adopt that idea myself, in preference to my own, which was that plants imbibe the phlogiston with which the air is overcharged *into their substance*, and convert it into their proper nourishment. But when I tried the effects of steam on phlogisticated air, with as much attention as I could give to the experiments, I never found that it was at all mended by the process.

I first took a quantity of air that had been phlogisticated by a mixture of iron filings and brimstone, and introducing into it the end of a glass tube, communicating with a phial, which I had filled with water, I kept it in a boiling heat, about a quarter of an hour, in which time the steam had effectually pervaded the mass of air, having made

made the jar in which it was contained thoroughly hot, and having expelled three fourths of it. But what remained of this air was no more diminifhed by nitrous air than it had been before.

Afterwards I feveral times filled jars with air phlogifticated with nitrous air, and alfo by other means; and placing them, inverted, in pans of water, made the water boil a long time, till a great part of the air was expelled by the fteam, but I never found the air fo expofed to fteam to be at all mended by it. Common air was always fenfibly injured by this procefs, as might have been expected from my former experiments.

I am willing to think, however, from the obfervation of Mr. Arden, an intelligent lecturer in natural philofophy, who firft mentioned the obfervation to me, as his own, that fteam, or the vapour of water, may unite with fomething or other that makes air offenfive, and help to fweeten it; or, at leaft, that throwing a quantity of fteam into a room in which the air is offenfive may promote a change of the air, fo as to be an eafy and valuable remedy in fuch cafes. He has mentioned to me feveral experiments of his own, as well as obfervations of other perfons, that make it very probable.

6. *Of the Effect of the* Electric Spark *on common Air.*

In the preface to my third volume of *Obfervations on Air*, I mentioned the refult of feveral experiments on taking the *electric fpark* in common air. I have fince purfued this fubject a little farther, with a view to fome peculiar circumftances attending the diminution of the air in this procefs, and the depofit of an acid from it. But before I recite the obfervations, I cannot help expreffing my concern that feveral perfons have not been able to fucceed in the fimple experiment of the diminution of air by the electric fpark, and changing the colour of the juice of turnfole over which the diminution is made. For the fatisfaction of fuch perfons, I fhall recite all the cir-

circumſtances neceſſary to be attended to in it, as I repeated the experiment in the preſence of Mr. Magellan and Mr. Nairne, who carefully attended to the whole proceſs.

Having nearly filled a glaſs tube about a tenth of an inch in diameter, open at one end, and having a piece of iron wire cemented in the other, with water tinged blue with the juice of turnſole (having previouſly expelled the air by means of an air pump, ſo as to leave about three fourths of an inch of air in the tube) we took the electric ſpark in it, till the air was conſiderably diminiſhed, and the liquor turned red. We then expelled the red liquor by means of the air pump, expanding the air, and admitted more blue liquor; and then we repeated the electrification till the diminution had proceeded as far as it would, which was about one fourth of the whole bulk of the air. Then, admitting the blue liquor again, the machine, which was a very powerful one (conſtructed by Mr. Nairne for Lord Shelburne) a full half hour, without being able to effect the leaſt farther diminution of the air, or the leaſt ſenſible change in the colour of the blue liquor. They were both ſatisfied that no experiment could be made with greater fairneſs.

I ſhall now proceed to mention other circumſtances attending this proceſs.

I took the electric ſpark in common air confined by quickſilver; and then, admitting to it water tinged blue with the juice of turnſole, it became red in the ſpace of a day and two nights, but the colour did not change preſently. Alſo, after this the diminution was greater than it had been before.

Having taken the electric ſpark in common air upon quickſilver, as before, it was preſently diminiſhed as uſual; and the next day without any farther electrification, the diminution was more conſiderable. The third day I admitted to it the juice of turnſole, and in about an hour it appeared to be red at the top, but was not ſenſibly diminiſhed more than before. In leſs than a day a day it became wholly red, and then no farther diminution was apparent.

I took a quantity of water which had been made blue with the juice of turnſole, and which had been made ſeemingly red with the electric ſpark, taken in the common air over it; but, on mixing all the parts of it together, it reſumed its blue colour (the blue colouring matter having only ſubſided to the bottom) ſo that alteration in the conſtitution of this liquor by this proceſs, though manifeſt to the eye, is not, in fact, ſo very conſiderable. It is evident, however, from the preceding obſervations, that it could not be the mere *concuſſion* given to the air by the ſpark, or ſhock, that had this effect upon it; becauſe when the air was completely diminiſhed, the ſpark or ſhock had no effect, and the liquor turned red when it was admitted to the air a long time after the operation of the electric ſpark upon it, while it was confined by quickſilver. This circumſtance may deſerve farther inveſtigation.

7. Of

7. *Of the effect of the Calces of Copper and Iron on Air.*

Several properties of *metallic calces* may be diſcovered by their expoſure to the common air. I have made ſome obſervations which may be pleaſing and ſatisfactory with reſpect to thoſe of *copper* and *iron*. They prove that the blue colour acquired by the former, and the red colour acquired by the latter, are owing to the dephlogiſtication of them. For theſe colours cannot be aſſumed by them but in the open air, and the air to which they are expoſed is more or leſs phlogiſticated by this means.

I diſſolved copper in a ſolution of ſal-ammoniac, and confined the ſolution in a phial with a ground ſtopper. After a day or two, when the ſolution was become thoroughly blue, I examined the air within the phial, and found it to be conſiderably worſe than it had been. For one meaſure of it and one of nitrous air occupied the ſpace of 1.33 meaſures; when the common air at the ſame time was diminiſhed by the

the nitrous air ſo much, that the ſame quantities occupied the ſpace of little more than 1.1 meaſures. At another time I covered a phial containing a quantity of this ſolution with a ſmall jar ſtanding in a trough of water, and found, after a few days, though not more than half the ſolution, beginning from the top, had turned blue, that the air to which it had been expoſed was almoſt completely phlogiſticated.

Pouring a diluted ſolution of pearl aſhes into a diluted ſolution of *green vitriol* with a funnel, that the common air within the phial might mix as little as poſſible with the open air, the precipitate was at firſt of a light blue; but by expoſure to the air it became firſt of a deep indigo blue, and then a red.

Covering a quantity of this blue precipitate contained in a glaſs cup, with a glaſs jar ſtanding in water, I obſerved that, after two or three days, all the ſurface of the precipitate, though covered with water, was become red. When I ſtirred it up, all below the ſurface was as blue as ever. In this ſtate I examined the air, and found it ſenſibly phlogiſticated, though not to a great degree.

Having made another blue precipitate of iron, I poured it into a ſmall retort, and turning it every way, to give all the inſide a coating of it, I expoſed it to the heat of the fire, till it was become partially red (for I did not perceive it would become wholly ſo) and, examining the air in the inſide, I found that one meaſure of it and one of nitrous air occupied the ſpace of 1.3 meaſures; when the ſame quantities of common air and the ſame of nitrous air occupied the ſpace of 1.24 meaſures.

Laſtly, to give the calx of iron more time to affect the air, I made the mixture in a phial which I left half full of air; and in a few days the ſurface of the water was covered with a red pellicle, and ſome time afterwards the ſurface alſo of the precipitate at the bottom of the phial, which had been of a deep blue, was become red. After waiting three weeks, I examined the air, and found it ſo much phlogiſticated, that

that one meaſure of it and one of nitrous air occupied the ſpace of 1.55 meaſures.

Having alſo coated the inſide of a glaſs tube with the green precipitate, I let it ſtand near three weeks with its orifice immerſed in water, in which time it had become nearly red; and then examining the air, I found no fixed air in it (which might have been ſuſpected to come from the pearl aſhes eſpecially; and thus to have injured the air, without any proper phlogiſtication) and one meaſure of it and one of nitrous air occupied the ſpace of 1.45 meaſures. In this experiment, therefore, there was a proper phlogiſtication of the common air, without any thing from the alkaline ſalts.

It is not a little remarkable, that this change of colour will take place though the precipitate be covered with a large body of water. I have found it when it was covered to the depth of eleven inches, which is that of the trough in which I uſually make my experiments. It was at firſt all blue, the next day I found the ſurface completely red, when the bottom was as deep a blue as ever. This reſembles the property of *ſerum* in my experiments on *blood*. For as that liquor admits phlogiſton to paſs from the blood to the air, ſo water permits phlogiſton to paſs from this precipitated calx to the air.

The reſult of theſe experiments will be different according to the degree of ſaturation in the ſolution, and perhaps according to other circumſtances.

At the ſame time that I got the deep blue precipitate, with which I made ſeveral of the experiments above-mentioned, I mixed a quantity of the ſaturated ſolutions, both of the vitriol and of the pearl aſhes, in an open jar, and the whole became red at once, without my being able to perceive any previous blue colour at all. Sometimes the precipitate will be white, or grey, eſpecially when the ſolution of the iron is poured into that of the alkali. In this caſe the firſt change is to a very light blue, then to a deeper blue, and laſtly to a red.

In

In the laſt experiment above-mentioned the air became phlogiſticated in conſequence of the liquor to which it was expoſed *acquiring* colour; whereas in the following it was injured at the ſame time that the liquor *loſt* its colour. I took a quantity of ſpirit of ſalt made yellow by various impregnations, and then made it colourleſs by liver of ſulphur. After this I inverted the phial with common air in it, and let it ſtand about a week, obſerving that in two days it had recovered its original yellow colour; and the air appeared to be ſo much injured, that one meaſure of it and one of nitrous air occupied the ſpace of 1.9 meaſures. The phlogiſton that produced this effect came probably from the liver of ſulphur.

8. *Air injured by the Effluvium of Water freſh diſtilled.*

Notwithſtanding it has been a maxim with chemiſts, that water contracts no union with phlogiſton, it is acknowledged that water freſh diſtilled acquires ſomething of an empyreumatic nature, which gives it an unpleaſant flavour, and which goes off by expoſure to the open air. That this volatile principle is phlogiſton I aſcertained by expoſing air to the influence of it.

I took water freſh diſtilled in copper, and filled a phial about half full of it, and examining the air within the phial about a month afterwards, I found it ſo much phlogiſticated, that one meaſure of it and one of nitrous air occupied the ſpace of 1.32 meaſures; when, with the ſame nitrous air and common air, the ſame meaſures were 1.22.

In this caſe it might be ſuſpected that the phlogiſton came from the copper. But at the ſame time I made a ſimilar experiment, with a ſimilar reſult, on water diſtilled in glaſs. In this caſe there was more air, and a ſmaller quantity of water in the phial, but the time of expoſure was nearly the ſame; and with this air the meaſures of the teſt were 1.26. It is probable that with more water, more time, and leſs air, the reſult would have been more conſiderably in favour of the water having acquired phlogiſton

phlogiſton in the act of evaporation, without any communication with ſubſtances that are thought to contain it. This experiment, however, is ſufficiently ſimilar to the others I have recited, in which mere *heat* had the ſame effect as the communication of phlogiſton. But water, in this caſe, like the ſpirit of nitre in the former, might contain phlogiſton, and the evaporation might change the *mode of its combination*, ſo as to make it more eaſy to be imparted to air.

SECTION XXVIII.

Obſervations relating to the Melioration of Air by the GROWTH OF PLANTS.

IN my firſt publication on the ſubject of air, I gave an account of ſeveral experiments by which it appeared that air injured by reſpiration, putrefaction, or the burning of candles, was unqueſtionably reſtored to a great degree of ſalubrity by the growth of ſprigs of mint, and other plants in it. At the ſame time I mentioned other inſtances, in which, to my great ſurprize, air, which I had imagined, from the appearance of the plants growing in it, muſt have been in a mending ſtate, had not grown better at all, and had ſome times grown much worſe. See Vol. I. p. 91, &c.

Of the reſtoration of air in which candles had burned out to a ſtate in which they burned very well in it again, I had many inſtances in the years 1771, and 1772, in the latter without a ſingle failure,

p. 53; and in the former year I recollect not more than one, not mentioned in my account, becaufe it was but one out of very many, and might eafily be accounted for without affecting the conclufion which I then drew from the whole, *viz.* that it is *very probable*, that the injury which is continually done to the atmofphere by the refpiration of fuch a number of animals as breathe it, and the putrefaction of fuch vaft maffes both of vegetable and animal fubftances expofed to it, is, in part at leaft, repaired by the vegetable creation. For if a plant be unhealthy, or if a few leaves drop off and putrefy, it will not only prevent the reftoration of the air, but will contribute to make it worfe. On account of this fingle failure, however, I did not make any conclufion, not even in favour of the *probability* of my hypothefis, till the year following, in which it fo happened that I had not one failure.

Probable, however, as I thought it to be from the whole of my experiments, that vegetation tends to counteract the noxious effects

effects of reſpiration, putrefaction, and the burning of inflammable ſubſtances, by plants inhaling the phlogiſton thrown into the air by theſe proceſſes, I conſidered the ſubject (ſee p. 92) as "well deſerving a "farther inveſtigation, as it might throw "light on the principles of vegetation." Such, however, has been my ſituation and engagements ſince that time, that till the year 1777, I never repeated any of my former experiments on this ſubject, though I always had it in contemplation, and meant to proſecute them much farther than I had done before.

Having heard that ſeveral perſons abroad had not been able to repeat my experiments with the ſame ſucceſs, I now reſumed them; and when I had made ſome progreſs in them I heard of the experiments of Mr. Scheele on beans, who reports the reſult of them to have been conſtantly the reverſe of mine. On this account I gave the more attention to this buſineſs in the ſpring and ſummer of 1778; and though I was interrupted in the proſecution of them,

I made

I made a confiderable number in the beginning of the fummer, the refult of which was as follows.

1. In general, the experiments of this year were unfavourable to my former hypothefis. For whether I made the experiments with air injured by refpiration, the burning of candles, or any other phlogiftic procefs, it did not grow better but worfe; and the longer the plants continued in the air, the more phlogifticated it was. I alfo tried a great variety of plants, but with no better fuccefs, as fprigs of mint, fpinach, lettuce, onions, brooklime, and fome others. The method in which I ufed them was, generally, to put the roots into phials filled with earth and water, and then to introduce them through water into the jar containing the air on which I was making the experiment; the jars being about ten inches in length, and two and a half in diameter.

2. I have had feveral inftances of the air being undoubtedly meliorated by this procefs, efpecially by the fhoots of ftrawberries,

berries, and ſome other plants, which I could, by bending, introduce into the jars or phials of air, ſupported near them in the garden, while the roots continued in the earth. This I thought to be the faireſt method of trial, the plant growing, in every reſpect, in its natural way, except that part of the ſtem was obliged to lie in water, and the ſhoot was in air, confined in a narrow jar.

3. I had other inſtances, no leſs unqueſtionable, of common air not only receiving no injury, but even conſiderable advantage from the proceſs; having been rendered in ſome meaſure dephlogiſticated by it, ſo as to be much more diminiſhed by nitrous air than before; a thing which I was far from expecting; having had nothing farther in view than ſimply to try whether the air would be injured or not; Mr. Scheele, who made his experiments with beans, having always found it injured.

4. In moſt of the caſes in which the plants failed to meliorate the air, they were

were either manifeftly fickly, or at leaft did not grow and thrive, as they did moft remarkably in my firft experiments at Leeds; the reafon of which I cannot difcover. Indeed, I did not at this time make ufe of any air tainted with putrefaction, contenting myfelf with that which was injured by my own refpiration, or the burning of candles; and it was in air tainted with the putrefaction of animal fubftances that my plants had flourifhed the moft. As to air injured by other proceffes, as by iron filings and brimftone, or by nitrous air, I had not made trial of it before, except the latter, which I exprefsly faid (Vol. I. p. 119) did not fail to kill the plant.

In thofe inftances in which the plants grew the beft, they were, however, but fickly, as appeared by the leaves foon turning yellow, and falling off when the leaft motion was given to them. In fome cafes, however, as in thofe mentioned in Vol. I. p. 91, I faw no particular reafon why the air fhould not have been meliorated.

Upon the whole, I ftill think it *probable* that the vegetation of healthy plants, growing in fituations natural to them, has a falutary effect on the air in which they grow. For one clear inftance of the melioration of air in thefe circumftances fhould weigh againft a hundred cafes in which the air is made worfe by it, both on account of the many difadvantages under which all plants labour, in the circumftances in which thefe experiments muft be made, as well as the great attention, and many precautions, that are requifite in conducting fuch a procefs. I know no experiments that require fo much care. Particularly, every thing tending to putrefcence, every yellow or ill-looking leaf, &c. muft be removed, before the air can have been injured by it, and I did not at this time watch my plants with fo very much attention as I did when I firft made my experiments; though the method I now ufed in *examining* the ftate of the air was much more exact than any that I was acquainted with at that early period of my obfervations on air.

It

It was in June 1772, that I firſt made nitrous air, and it was conſiderably later in that year that I diſcovered its property of ſerving as a ready teſt of the purity of common air; whereas my experiments on plants were begun in 1771, and were reſumed in June 1772. Alſo, after I had diſcovered the uſe of nitrous air, as a teſt of the purity of other air, it was ſome time before I hit upon any tolerably exact method of applying it; and indeed before I had perfect *faith* in it; which will not be thought extraordinary by any perſon verſed in theſe matters, or indeed acquainted with human nature in general. We always queſtion every new fact, or hypotheſis, and more ſo in proportion both to its novelty, and importance. We are, therefore, ſeldom quite ſatisfied ourſelves, till we have had an opportunity of ſatisfying other perſons with reſpect to them. Now, it was not till the cloſe of that year, when my experiments on plants were nearly brought to a concluſion, that I obtained that complete ſatisfaction with reſpect to this capital uſe of nitrous air.

air. Accordingly, it may be obſerved, that the teſts I then made uſe of were the ſame that I had always uſed before, *viz.* the burning of candles, and the reſpiration of mice, in the application of which I had acquired a greater degree of dexterity and exactneſs than can well be imagined; at which my friends were often much amuſed, and myſelf, of courſe, not a little pleaſed. On all theſe accounts, I contented myſelf with the more inaccurate methods of aſcertaining the purity of air, and made but little uſe of the better method, which I had but lately diſcovered; though I did not wholly neglect this method, eſpecially in caſes of much conſequence, as may be ſeen Vol. I. p. 90, 91, 92, *&c.*

After theſe obſervations, I think it will be unneceſſary to recite the particulars of thoſe caſes in which the growth of plants failed to reſtore any ſpecies of noxious air. But, for the reaſon mentioned above, it will be of conſequence to be as particular as I can with reſpect to thoſe inſtances in which it ſucceeded.

On

On the 28th May I introduced a ſhoot of a ſtrawberry plant into a jar containing air vitiated partly by the burning of candles, and partly by other means, till one meaſure of it and one of nitrous air occupied the ſpace of 1.62 meaſures; and on the 10th of June this air was ſo far improved, that when it was tried in the ſame manner, the meaſures of the teſt were 1.4, and a candle did not immediately go out in it.

June 29th, a quantity of air which was perfectly noxious, not being in the leaſt diminiſhed by nitrous air (having been firſt injured by the burning of candles, and afterwards by plants confined, and perhaps putrefying in it) on the 23d of the ſame month, was ſo far reſtored by a ſtrawberry ſhoot, that one meaſure of it and another of nitrous air occupied the ſpace of 1.62 meaſures.

At the ſame time another quantity of air which had been quite noxious, and in which a ſprig of winter ſavory had grown the ſame time, was ſo far reſtored, that equal meaſures of this and nitrous air occupied the ſpace of 1.64 meaſures.

July 1ſt, the air of a jar in which a candle had burned out, and in which a ſtrawberry ſhoot had grown from the 23d of June, was ſo much improved, that the equal meaſures above-mentioned occupied the ſpace of 1.24 meaſures; when thoſe in which the common air of the garden was uſed were 1.3.

July 5th, air which had been ſo noxious, that the meaſures of the teſt were 1.64, on the firſt of the ſame month, and in which a ſtrawberry ſhoot had grown in the mean time, were 1.56.

At the ſame time air in which a candle had burned out (by which I found air was ſo far injured that equal meaſures of it and of nitrous air occupied the ſpace of 1.44 meaſures) and in which a very ſmall ſprig of parſley, in proportion to the ſize of the veſſel, had grown from the firſt of the ſame month, was ſo far reſtored, that when it

was

was examined, the meaſures of the teſt were 1.29. Alſo, in another quantity of the ſame air, in which a ſtrawberry ſhoot had grown the ſame time, the meaſures were 1.34

In all theſe inſtances, it will be obſerved, that plants grew in the common ſoil of the garden. I had but one inſtance of any melioration of air in other circumſtances, and that was not conſiderable.

July 6th, one meaſure of air in which a candle had burned out, and in which a young bean had afterwards grown ſeven days, and another meaſure of nitrous air, occupied the ſpace of 1.385 meaſures; when with the common air, and the ſame nitrous air the meaſures were 1.275. With air in which a candle had burned out, the uſual meaſures of the teſt were, as I have obſerved before, 1.44. In all the other caſes of beans growing in confined air, the air was either made worſe, or at leaſt not mended.

All the caſes in which common air was improved by vegetation were thoſe in which the roots of the plant were in the ground,

 and

and flexible ſprigs from them were bent, and made to paſs through a body of water into the jars or phials containing the air. But there was this advantage in this caſe, that I had no occaſion to draw the whole ſprig through the water, but only to place the inverted jar over it, pouring water into the baſon in which the jar was placed, in order to cut off the communication with the external air. But that this method did make an effectual ſeparation between the air within the jar and the external air, was ſufficiently evident from the reſult of thoſe experiments, in which the air within the jar was better than the common air; and therefore the ſame method may be depended upon in the preceding experiments.

The firſt inſtance that looked like the melioration of common air by vegetation occurred the 16th of June, when I examined the air in which two different ſhoots of ſtrawberry plants had grown from the 11th of the ſame month. Though theſe plants had grown very poorly, and the leaves were not ſenſibly expanded, the air, I obſerved, was

was rather better than worſe, though not ſo much ſo, as that I could be quite ſure of the fact. But the next day I obſerved that the air in another jar, in the ſame circumſtances with the former, was certainly rather better than common air; though ſtill I ſhould not have drawn any general concluſion from it, if it had not been confirmed by other more deciſive obſervations.

On the 21ſt of June I had, however, indiſputable evidence of the melioration of air in which a plant had grown. It was a ſprig of *winter ſavory*, and it had grown in the jar from the 16th of the ſame month. The improvement of this air, meaſured in the uſual manner, was in the proportion of 1.275 to 1.375. I had air enough for three trials of it, and the reſult was the ſame in them all.

June 26th, common air in which a ſprig of parſley had grown very well from the 16th was ſo pure, that one meaſure of it and one of nitrous air occupied the ſpace of 1.14 meaſures; when equal meaſures of the beſt common air and the ſame nitrous air meaſured, at the ſame time, 1.29. I immediately replaced the ſprig in the ſame air, and on the 6th of July I examined it again, when the air was ſtill more improved; the meaſures of the teſt being exactly 1. This reſult was very clear, and certainly remarkable.

June 29th, a jar of common air in which the ſhoot of a ſtrawberry plant had grown from the 17th of the ſame month, was ſo pure, that the meaſures of the teſt were 1.18; when, with the common air, at the ſame time, they were 1.3. Alſo, on the ſame day, the common air in which a ſprig of winter ſavory had grown the ſame time was improved in the ſame proportion. But in this jar there was a little of that *green matter*, which, as will be ſeen hereafter, uſually attends the ſpontaneous production of the pureſt air.

When theſe obſervations are well conſidered, I think it will hardly be doubted, but that there is ſomething in the proceſs of vegetation, or at leaſt ſomething uſually

attending

attending it, that tends to meliorate the air in which it is carried on, whatever be the *proximate cauſe* of this effect, whether it be the plants imbibing the phlogiſtic matter, as part of their nouriſhment, or whether the phlogiſton unites with the vapour that is continually exhaled from them; though, of the two opinions, I ſhould incline to the former.

The action of a plant conſidered as ſimply *vegetating* in air is a thing quite different from the effect that the *exhalation of the flower*, and perhaps other particular parts of the plant, may have upon it. *Smell*, the old chemiſts ſaid, was an indication of phlogiſton, and I find that the moſt delicate flowers injure the air much more than I had imagined. Nothing is ſweeter than a *roſe*, and yet the fragrant effluvia of it is far from being favourable to the air in which it is confined.

On the 25th of June I confined a full blown red roſe in about four ounce meaſures of common air, having covered it with a ſmall glaſs jar ſtanding in water;

and I obſerved that, the next day, the air was ſo much injured, that one meaſure of it and another of nitrous air occupied the ſpace of 1.75 meaſures; ſo that I doubt not that any animal would have expired immediately on being put into it. The day following the meaſures of the teſt were 1.9, and the day after ſomething more. Notwithſtanding this, when the roſe was withdrawn, it did not ſeem to have loſt any thing of its agreeable fragrance.

SECTION XXIX.

Of the State of Air confined in the Bladders of Sea Weed.

I WAS much confirmed in the hypothefis of vegetation reftoring atmofpherical air to a ftate of greater purity, by finding the air within the bladders of the common *fea weed* to be confiderably better than the common external air. This was a cafual and unexpected obfervation that I made in the courfe of the laft fummer at Lymington, and I wifh that fome philofophical perfons who live near the fea would examine this circumftance a little farther, both for the fake of inveftigating the *origin* of this air, and the particular oeconomy of the plant that contains it. It might even lead to fome farther knowledge of the ftructure of plants in general.

Before I recite thefe obfervations, I would remind my reader, that I formerly gave fome attention to the air contained in the hollow

hollow parts of certain plants, particularly the *bladder fena*, and the ftalks of onions; but, in thofe two cafes, I found the air, as far as I could then judge, not to differ from that of the furrounding atmofphere. This being an obfervation of no confequence, I defifted from profecuting it, imagining there muft be fome eafy communication between thofe cavities in plants and the external air, fo that much difference could not be expected. I found, however, in the courfe of this fummer, that, in two other cafes, air fo confined was much inferior in purity to that of common air.

Air preffed out of the ftalks of the common *flag* (as I think it is called) growing in water, was in fuch a ftate, that one meafure of it and one of nitrous air occupied the fpace of 1.5 meafures. And air in the infide of a plant refembling hemlock was even worfe than this. For when I examined it I found the meafures of the teft to be 1.75.

Upon this I was rather inclined to fuppofe, that if the air within the cavities of plants was examined with rigour, it would always be found rather worſe than the air in the ſurrounding atmoſphere, eſpecially if the plant was in the ſmalleſt degree unhealthy; as the phlogiſton diſcharged in any tendency to diſeaſe would eaſily affect the air of ſuch cavities; and there being no viſible circulation, it would probably retain ſuch a taint a conſiderable time. Though I might have ſuppoſed, that if the plant was very healthy, and did imbibe phlogiſton from the neighbouring air, the air in thoſe cavities (in what manner ſoever it came there) would be *depurated* by that means, and thereby approach to the ſtate of dephlogiſticated air. This may perhaps be the caſe with the air in the bladders of ſea weed, though I could wiſh to know a little more concerning the origin of this air. For as ſome of the plants grow intirely under water, there is no appearance of this air having ever been atmoſpherical air, but rather of its being generated within the plant itſelf.

I ob-

I obſerved three kinds of this ſea weed, one which I take to be the *quercus marinus*, the bladders, when full grown, being about half an inch in diameter, and rather of an oval form; another in which the bladders were ſpherical, about a quarter of an inch in diameter; and a third in which the bladders were much larger than theſe, being formed by the ſeparation of two *laminæ* of which the plant conſiſts, ſo as to reſemble a fillet, the bladder being exactly of the breadth of the flag, and rather longer than it is broad.

The firſt of theſe was moſt common on the ſea ſhore at Lymington. The firſt that I took up had lain a conſiderable time on the ſhore, ſo that the bladders were become very hard and brittle, and the air within them was exactly in the ſame ſtate with the air of the atmoſphere. But afterwards, on the 25th of July, I happened to meet with a quantity of this weed that had juſt been thrown up by the ſea, quite moiſt, and the bladders ſoft. Burſting them under water, and examining the air, I found that one

meaſure

meaſure of it and one of nitrous air occupied the ſpace of not more than one meaſure; whereas, when I applied the ſame teſt to the common air, the meaſures were 1.3.

This degree of purity ſo far exceeded my expectations, that, though I made the experiment with all the attention that I was capable of, I could not help ſuſpecting that I had, unperceived by myſelf, let ſome of the air eſcape as I was mixing it; and I acquieſced in this idea ſome time, in conſequence of having found the air within ſome of the largeſt of the bladders abovementioned, even when plucked up from the roots with my own hands no better than common air. But many of theſe bladders were old, and quite black, growing upon a beach where they were not intirely covered by the ſea, even at high water; ſo that the bladders not being always moiſt, there might have been ſome communication with the external air; and the inſide of ſuch bladders, if they ſhould happen to be in a ſtate of decay, would

 greatly

greatly contaminate and deprave the air contained in them.

On the 29th of July, I gathered a quantity of this weed containing the largeſt bladders, and alſo of that which had the ſmalleſt. At that time the air in theſe did not differ from the common air. Of the large bladders I ſeparated the black ones from the reſt; and, preſſing out the air contained in them, I found that one meaſure of it and one of nitrous air occupied the ſpace of 1.2 meaſures; but with the air from thoſe bladders which were not turned black, the meaſures were 1.06.

Theſe obſervations were ſtill in favour of the greater purity of the air contained in theſe bladders; but I was thrown back into my former doubts, by finding, preſently after, the air in the bladders of ſome weed that I took up quite freſh and moiſt on the ſea ſhore, did not differ at all from the common air. * Had I happened to have met

* I might have conſidered that, though theſe weeds were *moiſt*, in conſequence of having been lately thrown up by the ſea, it might have been a long time ſince they were in a ſtate of vegetation.

met with this weed at the firſt, I ſhould certainly have examined into the matter no farther; but having had different reſults in my former trials, I was not willing to leave the ſea with my doubts unſatisfied; and for this purpoſe I went to the ſea ſhore at low water, and gathered the plants that were then growing in the water; ſo that they could never have been dry; and the air within the bladders could never have had any communication with that of the atmoſphere. I gathered thoſe plants only that were ſeemingly young and freſh; and I took a great quantity of them, ſo as to have air enough for many experiments; and being determined to abide by the reſult of this trial, I proceeded to the examination of it with the greateſt precaution; when I found, in three different trials, that one meaſure of this air and one of nitrous air occupied the ſpace of 1.1 meaſures. At the ſame time, with the ſame mixtures of common air, the meaſures were 1.35.

I could have wiſhed to have examined the air of theſe weeds from the firſt formation

tion of the bladders and at different times of the year, &c. but I muft leave this to be farther inveftigated by thofe who have better opportunities for the purpofe.

SECTION XXX.

Of the Property of the Willow Plant to abforb Air.

OF the various plants on which I made experiments in the courfe of this fummer, I met with one which had the remarkable quality of abforbing a great proportion of any kind of air to which I expofed it. It is the *epilobium hirfutum* of Linnæus, in Englifh the *willow plant*, and it grows beft in the water of marfhy ground. The method in which I made the experiments was by fixing the jar of air with its mouth in the water in which the plant grew, keeping it upright, by faftening it to a ftick fixed in the bottom of the pool, then bending the plant under the water, and

and introducing the top of it into the inside of the jar.

I presently found that the common air to which it was exposed in this manner was considerably diminished, and rendered noxious; but having neglected one of these jars for about a week, I was surprized to find that near one half of the whole quantity of air was absorbed, the water having risen so far within the jar; whereas, in general, the diminution of air occasioned by what I suppose to be mere phlogiston, as in the process of iron filings and brimstone, or the calcination of metals, &c. does not exceed one fourth of the whole. Supposing, however, that I might not have taken sufficient notice of the quantity of air originally contained in the jar, I repeated the experiment in a jar about ten inches long, and one in diameter, and found, after some time, that the diminution went unquestionably beyond one fourth of the whole; and then, to prosecute the the experiment farther, I introduced other plants of this kind into jars about nine inches in length, and $2\frac{1}{4}$ in diameter, one of them filled with inflammable, and the other with nitrous air.

After about a fortnight, I noted the ſtate of theſe plants, and of the air to which they were expoſed, and found them to be as follows: The plant which had been expoſed to common air, in the jar about ten inches long and one inch wide, and which had been, in all, about a month in that ſituation, had abſorbed ſeven eighths of the air in the jar. The plant was quite yellow and dead; but though it had been ſo for ſome time, it had ſtill continued to abſorb the air.

The plant which had been confined only about a fortnight, in one of the larger jars of common air, was quite green, and had conſumed three fourths of it.

The plant in a jar of the ſame ſize, containing inflammable air, had conſumed one third of it, and part of the remainder (which I drew from it) was, to all appearance, as inflammable as ever it had been. The plant was green.

The

The plant in the nitrous air was yellow and dead, and had conſumed one third of its air.

In this ſtate I was obliged to leave theſe plants, and to ſuſpend all my other experiments on plants by my journey to the ſea ſide; but I had accounts ſent me of the ſtate of them from time to time, by which it appeared, that the air continued to diminiſh till the common air in the narrow jar was only one tenth of its original quantity, the inflammable air was reduced to one ſeventh of the whole; and the air in the other jars was diminiſhed in about the ſame proportion. But at length, the ſummer being very dry, the water failed, and the common air, of courſe, got into the jars. I regret, particularly, that I had no opportunity of examining the ſtate of the *inflammable* air in the laſt ſtage of its diminution.

Finding this plant to abſorb ſo much air, I was deſirous of knowing what became of it, whether it was incorporated in the ſubſtance of the plant, or was merely ſtrained through it. For this purpoſe I put the root of one of the plants, with all the earth that adhered to it, into a jar; and bending the plant a little, placed the jar in ſuch a manner, as that the mouth of it was juſt immerſed in a pan of water, and the plant, though in an aukward ſituation, grew pretty well; the upper part being ſupported, and alſo turning upwards of itſelf, by its natural growth.

Some air was certainly ſtrained through this plant; but much leſs than I had expected, conſidering the quantity that I ſuppoſed it would have abſorbed in the ſame time, at leaſt if it had grown freely in its natural ſituation. The air which I collected in this manner was almoſt intirely phlogiſticated, as was always that which remained of the common air that the plant had abſorbed.

To try whether the plant would actually abſorb air in the ſituation above deſcribed, when the root was confined in a jar of water, I gave it another bend near the top, and placed a jar of common air over it,

ſtanding

ſtanding in another veſſel of water; but the plant would not bear ſo much torture, and though it did not die immediately, it decayed gradually, and the experiment had no effect.

It will certainly be well worth while to compare all the circumſtances in which air is *abſorbed*, as well as thoſe in which it is merely *diminiſhed* to a certain degree, in order to aſcertain the circumſtances that are common to all the caſes, and thereby diſcover the proper cauſe of this remarkable phenomenon. Water, and many other fluids, have this property in ſome degree, as has long been known to natural philoſophers, who did not give much attention to the *quality*, or *chemical properties* of air. I diſcovered it in a ſtill greater degree in *oil of turpentine*. See Vol. III. p. 92; and that excellent philoſoper the Abbé Fontana has diſcovered it in a much greater degree ſtill in *charcoal*. This plant, however, ſeems to poſſeſs the ſame property in as great a degree as charcoal. It only requires more time to produce its effect. At another opportunity I propoſe to examine this matter a little farther. At preſent, no conjecture occurs to me that I think worth communicating to the public.

SECTION XXXI.

Of the Growth of Plants in Dephlogiſticated Air, compared with their Growth in other Kinds of Air.

IT will be allowed to be an argument favourable to the hypotheſis of vegetation repairing the injury done to the air by reſpiration and putrefaction, that plants do not grow ſo well in dephlogiſticated air, as in common air. Of this I had ſome ſuſpicion from the ſingle experiment, the reſult of which is recited in my third volume p. 335; but I am now pretty well ſatisfied with reſpect to it, from experiments begun in April 1777, and continued occaſionally in the courſe of the ſummer following.

In

In order to compare the vegetation of plants in air differing as much as poſſible with reſpect to phlogiſton, I took three ſprigs of mint; and having put all their roots into phials containing the ſame pump-water, that had been ſome time expoſed to the open air, I introduced one of them into a jar of common air, another into one of dephlogiſticated air, and the third into air that had been phlogiſticated with nitrous air ſeveral months before. It was in ſuch a ſtate, that one meaſure of it and one of nitrous air occupied the ſpace of 1.75 meaſures. This was done in April; and examining the plants on the 12th of May following, I found that the plant in this phlogiſticated air had grown remarkably well, much better than that in the common air; whereas the plant in the dephlogiſticated air had a very ſickly appearance.

I examined theſe plants on the 26th of the ſame month, when the appearances continued nearly the ſame. And then, examining the ſtate of the air, I found that, though the plant in phlogiſticated air had grown ſo well, the air was not ſenſibly improved by it. The dephlogiſticated air was injured, which I attributed to the rotting of ſome of the leaves of the plant. The common air I did not attend to.

On the 7th of June following, I took an account of three ſprigs of mint, which had been growing, I believe, ſome weeks in dephlogiſticated air, and of three others, which had been growing the ſame time, and in all the ſame circumſtances in other reſpects in common air; and obſerved that, in all the three caſes, the appearances were deciſively in favour of the plants in the common air, the ſhoots being twice as large, and every other appearance of health in the ſame proportion.

I do not ſay that even theſe obſervations are quite ſufficient to determine the queſtion; but they ſeem to make it *probable*, that dephlogiſticated air does not ſupply that pabulam which plants derive even from common air; though I own it may injure them on ſome other account. Even Mr. Scheele, who maintains that vegetation has the ſame effect on air that reſpiration has, I find, allows that plants do not grow ſo well in dephlogiſticated as in com mon air.

SECTION XXXII.

Of the Growth of Plants with their Leaves in fixed Air, and their Roots in Water impregnated with fixed Air.

WHILE I was attending to the compariſon of the growth of plants in dephlogiſticated and common air, I at the ſame time made a few farther experiments on the growth of plants with their leaves expoſed to fixed air, though I was pretty well ſatiſfied, from the experiments recited Vol. III. p. 303, &c. that this kind of air is undoubtedly injurious to plants growing in it. I wiſhed alſo, once more, to try the effect of inflammable air with reſpect to vegetation.

Accordingly, in the ſame month of April 1777, I introduced a ſprig of mint into a phial of air one third fixed and the reſt common;

common; and having only once ſupplied it with freſh fixed air (when the bulk of the former was abſorbed by the water) I obſerved, that on the 3d of May following, there were black ſpecks on ſeveral of its leaves, and in the courſe of a week it was almoſt wholly black, and evidently dead. It had not grown at all.

At the ſame time I had put another ſimilar plant into a jar of half freſh made inflammable air and half common air, but it died preſently. I found, however, by ſubſequent trials, that plants would bear a greater proportion of inflammable than they would of fixed air; ſo that from the circumſtance of plants merely *living* in a proportion of fixed air, it cannot be inferred that it is *of itſelf*, at all favourable to their growth.

The few experiments that I had an opportunity of making, at the time of my laſt publication, left me altogether undecided with reſpect to the effect of water impregnated with fixed air on the *roots* of plants. See Vol. III. p. 320, &c. But the many experiments

riments that I have made ſince, in 1777, and 1778, have not left a ſhadow of doubt on my mind, that ſuch water is hurtful, and finally fatal to the plants growing in it, at leaſt to ſprigs of mint; for I did not make the trial with any other plants.

On the 28th of May I placed, in a green houſe, and not in my laboratory, as in the experiments mentioned in my third volume, three ſprigs of mint, with their roots in phials of water impregnated with fixed air, and three other plants of the ſame kind with their roots in the ſame water unimpregnated. After a week I changed the impregnated water, on account of the mouths of the phials being left open, leſt the plants ſhould have been injured by putting any thing about them, to prevent the eſcape of the air from the water.

During two or three days at the firſt, the plants in the impregnated water were more vigorous than the others; but on the 8th of June following, they all looked much worſe than thoſe in the common water. Alſo thoſe in the common water had long white filaments

filaments ſhooting from their roots, whereas thoſe in the impregnated water had none of them. On the 18th of June, the plants in the impregnated water were all quite dead, their leaves having all fallen off one after another, beginning at the bottom. Examining one of the phials, I found that it contained between one fifth and one ſixth of its bulk of fixed air.

I repeated theſe experiments ſeveral times in the courſe of that ſummer, generally uſing many more plants than in theſe laſt mentioned, but the reſult was the ſame in them all. However, as it generally happened, on what account I cannot tell, that the plants in the unimpregnated water died, though later than the others, I deferred the laſt and deciſive trial till the year following, after which I had no doubt remaining on the ſubject.

On the 4th of May 1778, I put ſeven ſprigs of mint into pump water impregnated with fixed air, and ten or twelve in the ſame water unimpregnated, the phials being ſimilar, and I placed them all in a ſummer houſe

houſe, in the ſame expoſure. I renewed the impregnated water every week, till the 23d of June, when all the plants in the water impregnated with fixed air were dead, the roots being black and rotten; while the other plants were in as flouriſhing a ſtate as poſſible, and continued to flouriſh long after, till I diſcharged the experiment.

On this occaſion I did not obſerve that the plants in the impregnated water were at any time more flouriſhing than the others, not even at the beginning; and after a fortnight the difference in appearance, to the diſadvantage of thoſe in the impregnated water, was very viſible. Thoſe which grew in the common water threw out many white filaments from their roots, many of them ſo long as quite to fill the phial, twiſting themſelves in all directions, and exhibiting a very beautiful appearance; whereas there was nothing of this kind in any of the phials of impregnated water. On the contrary, the roots became preſently black, and at length rotted quite away.

One of theſe I had overlooked, and had neglected to change the water; and this plant threw out a few white filaments; but, on renewing the impregnated water, they preſently became black and periſhed.

It was remarkable alſo, that two of the plants in the impregnated water threw out thick knots of thoſe white filaments in the necks of the phials, juſt above the ſurface of the water, but not one of them within the water itſelf, or ever entered the water. Alſo, when I took one of theſe plants, the roots of which were quite periſhed, out of the impregnated water, and put it into a phial of common water, it threw out new white roots above the place that was decayed, and afterwards grew very well.

Mr. Hey happened to ſee theſe plants in the laſt ſtage of the proceſs, and thought no experiment could be more ſatisfactory.

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SECTION XXXIII.

Of the ſpontaneous Emiſſion of dephlogiſticated Air from Water in certain Circumſtances.

PART I.

FEW perſons, I believe, have met with ſo much unexpected good ſucceſs as myſelf in the courſe of my philoſophical purſuits. My narrative will ſhow that the firſt hints, at leaſt, of almoſt every thing that I have diſcovered, of much importance, have occurred to me in this manner. In looking for one thing I have generally found another, and ſometimes a thing of much more value than that which I was in queſt of. But none of theſe unexpected diſcoveries appear to me to have been ſo extraordinary as that which I am about to relate; and it may ſerve to admoniſh all perſons who are engaged in ſimilar purſuits, not to overlook any circumſtance relating to an experiment; but to keep their eyes open to every new appearance, and to give

give due attention to it, how inconſiderable ſoever it may ſeem.

In the courſe of my experiments on the growth of plants in water impregnated with fixed air, I obſerved that bubbles of air ſeemed to iſſue ſpontaneouſly from the ſtalks and roots of ſeveral of thoſe which grew in the unimpregnated water; and I imagined that this air had percolated through the plant. It immediately occurred to me, that if this was the caſe, the ſtate of that air might poſſibly help to determine what I was at that time inveſtigating, *viz.* whether the growth of plants contributes to purify, or to contaminate the air. For if this air ſhould prove to be better than common air, I thought it would ſhow, that the phlogiſton of the imbibed air had been retained in the plant, and had contributed to the nouriſhment of it, while that part of the air which paſſed through the plant, having depoſited its phlogiſton, had been rendered purer by that means; though if the air ſhould not have been found better than common air, I ſhould not have concluded

cluded my hypothesis was false; since plants, like animals, might take in phlogiston in one state, and emit it in another.

With this view, however, I plunged many phials, containing sprigs of mint in water, laying them in such a manner, as that any air which might be discharged from the roots would be retained in the phials, the bottoms being a little elevated. In this position the sprigs of mint grew very well, and in some of the phials I observed a quantity of air to be collected, though very slowly; but I was much disappointed in finding that some of the most vigorous plants produced no air at all. At length, however, from about ten plants, I collected, in the course of a week, about half an ounce measure of air. This was the 19th of June 1778; and, examining it with the greatest care, I found it so pure, that one measure of it and one of nitrous air occupied the space of only one measure.

This remarkable fact contributed not a little to confirm my faith in the hypothesis of the purification of the atmosphere by vegetation; but I did not enjoy this satisfac-

 tion

tion long. For I conſidered that, if this was the proper effect of vegetation, it muſt be univerſal, and could not be confined to a few plants, eſpecially when others of the ſame ſpecies produced no ſuch effect. Beſides, when I removed the air-producing plants, as I thought them to be, into other and cleaner phials, I found that they yielded no more air than the other plants had done. And, what I thought more extraordinary ſtill, the phials in which theſe plants had grown, the inſides of which were covered with a green kind of matter, continued to yield air as well when the plants were out of them, as they had done before. This convinced me that the plants had not, as I had imagined, contributed any thing to the production of this pure air.

About the ſame time I obſerved that great plenty of air roſe ſpontaneouſly from the bottom and ſides of a tall conical receiver, about eighteen inches high, and five wide at the bottom originally made for the experiment of the fountain in vacuo, but which I had often uſed as a magazine for various kinds of air, and which was at that time employed

ployed for the ſame purpoſe; and both the plate on which it ſtood inverted, and the lower part of the receiver, were covered with this green matter.

To make my obſervations on this new ſubject of experiment with more attention, I transferred the air it had contained into another veſſel, filled the receiver with freſh pump water, and placed it where it had ſtood before, which was in a window on which the ſun ſhone; when air bubbles preſently began to riſe very faſt, ſo that, in three days, I had collected ſeven ounce meaſures, and this was ſo pure, that one meaſure of it, and two of nitrous air occupied the ſpace of four fifths of a meaſure.

Having found many of my phials which had the ſame green matter in them, I filled them alſo with freſh pump water; and, inverting them, I collected from them all conſiderable quantities of the ſame dephlogiſticated air, eſpecially when they were placed in the ſun; and it was very amuſing to watch them, and to obſerve the bubbles ſwell, and detach themſelves gradually from the green matter.

When I had advanced thus far in this intereſting inquiry, I was obliged to deſiſt from the farther proſecution of it, on account of a journey, on which I was abſent ſome months; and all that I could do was to leave a number of phials filled with different kinds of water, as river water, pump water, and rain water, with ſeveral other little varieties, in order to diſcover the circumſtances that were moſt favourable to the production of this green matter, whatever it was.

At my return, on the 8th of September, I found no green matter in any of the phials, excepting thoſe which contained pump water. Neither the rain water, or river water, had produced any. This pump water contains a conſiderable quantity of fixed air, and I muſt alſo obſerve that the inſides of the middle and lower glaſſes in one of Mr. Parker's apparatus's for impregnating water with fixed air were almoſt coated with this green matter.

After

After this I placed in my garden a large glaſs jar nearly filled with pump water, which I had ſtrongly impregnated with fixed air, and alſo jars of river water, rain water, and pump water unimpregnated; and on the 14th of October, I found almoſt all the bottom of the jar which contained the impregnated water covered with the green matter, but there was none at all in any of the other jars. This makes it probable, that the fixed air in the water contributes to the production of this matter.

That the external air, or animalcules in it, have nothing to do in the formation of this green matter, is evident from ſeveral of the preceding obſervations. This could not be the caſe, for inſtance, with the large inverted receiver, which had always yielded the greateſt quantity of this air, or with the water in the middle veſſel of Mr. Parker's apparatus. Beſides, at other times I have kept phials cloſely corked, and yet have found the green matter at the bottom of them, and it has yielded air plentifully, eſpecially in the ſun, or when placed near the fire. For when the matter is once formed, nothing but a certain degree of warmth ſeems to be neceſſary to its actual production of air.

The production of this green matter in cloſe veſſels ſeems to prove that it can neither be of an animal or vegetable nature, but a thing *ſui generis*, and which ought, therefore, to be characterized by ſome peculiar name; and all the obſervations that I have made upon it with the microſcope agree with this ſuppoſition. For, excepting a few filaments, that were hollow, and two or three globular pieces, perforated with ſome regularity, all the reſt of the ſubſtance ſeemed to be a congeries of matter of a compact earthy nature, the pieces ſeparately taken reſembling bits of jelly.

I have had ſome appearances, which, extraordinary as it will ſeem, make it rather probable, that *light* is neceſſary to the formation of this ſubſtance; but many more obſervations, which I believe can only be made in the ſummer ſeaſon, will be neceſſary to determine this. On the 23d of October,

October, I obferved that two fmall phials, which had been filled with pump water, and clofely corked on the 9th of Auguft preceding, had both of them a quantity of this green matter, while an open jar of the fame water, but in a much worfe light had none of it. There was, indeed, a greater depth of water in the jar than in the phials; and though I have generally obferved that this green matter is firft formed at the bottom of the veffels, it may poffibly require more time to the formation of it in proportion to the depth of water. Two other jars, however, about an inch deeper than that above-mentioned, and quite filled with the fame water, placed in the window on which the fun fhone had acquired this green matter, even in lefs time than the two fmall phials above-mentioned.

From *green*, this fubftance paffes gradually to a kind of *yellow*, or rather *orange colour*. For on the 14th of October, I obferved that the large receiver in which I had at firft collected a confiderable quantity of this pure air, and which I had always kept full of water, continued to yield air as copiouſly as ever, though both on the receiver itſelf, and on the plate on which it ſtood, the colour of this ſubſtance was quite changed to the orange colour above-mentioned.

On the 17th of September I had taken all the air from this receiver, and on the 14th of October following, on which I obſerved its change of colour, I took from it about nine ounce meaſures of air the very pureſt air I had ever got in this method. For one meaſure of it and two of nitrous air occupied the ſpace of 0,44 which is quite as pure as dephlogiſticated air at a medium.

PART II.

THE preceding part of this ſection was written while I imagined that the pure air I have mentioned in it was yielded by the *green matter*, which I have deſcribed, as depoſited from the water. But I preſently afterwards conſidered that the *formation* of the

the bubbles of air at the green matter was no proof that they were *yielded* by it; ſince no air, or even vapour, can iſſue from water, but at the place where it is bounded by ſome other ſubſtance; and the water might yield its air contiguous to one kind of ſubſtance in preference to another. Though, therefore, I had not perceived any bubble of air to iſſue from the water that had depoſited it, or from any part of the tranſparent glaſs, but only, as it ſeemed, from the green matter, I had been too haſty in concluding even that the water could not yield the air but with the aſſiſtance of that ſubſtance. At length the following experiment gave me juſt ideas on the ſubject.

Obſerving one of my phials of water that had got a coating of the green matter yielding air very copiouſly, I poured the water out of it into a clean phial, and found that, by the agitation given to it in the act of decanting, it ſparkled as much as any Pyrmont or Seltzer water. Inverting it in a baſon of water, I collected

lected the air, and found it to be very pure. I treated ſeveral other phials in the ſame manner, and the ſubſequent appearances being the ſame, I had no doubt but that when water is brought into a ſtate proper for depoſiting that green matter, it is, by the ſame proceſs, prepared for the ſpontaneous emiſſion of a conſiderable quantity of pure air. I therefore diſmiſſed all farther attention to the green matter, and ſhall leave it, after making the following obſervations.

I never found it except in circumſtances in which the water had been expoſed to *light*; and when, after ſtanding in the dark, the water has depoſited a whitiſh filmy matter, it has become green after a few days expoſure to the ſun. It was moſt freely depoſited from my pump water, and eſpecially when it had been impregnated with fixed air, but I have found it both in river water, and rain water, after longer ſtanding. I have generally found it at the bottom of the veſſel, but ſometimes it has been firſt formed at the top, and the coating from the

the bottom and that from the top meeting, the whole phial has acquired a coating of it from being once filled with pump water.

It is poſſible that, in ſome future time, I may examine farther into the nature of this matter, thus depoſited from water. But upon diſcovering that it was only a circumſtance preceding the ſpontaneous emiſſion of the air from the water, I gave attention to the *water* only, and to the relation it bore to the air contained in it, which is certainly not a little extraordinary; and when inveſtigated farther, will, I doubt not, appear to be a fact of the firſt conſequence reſpecting the doctrine of the atmoſphere.

I did not get this light into the buſineſs till it was too late in the laſt ſummer to make much uſe of *ſunſhine*, but I was aſſiduous enough to make all the uſe I could of ſuch weather as we had. And the general concluſion I have drawn from the whole that I was able to obſerve is, that whatever air is naturally contained in water, or in ſubſtances diſſolved in water, as calcareous matter, &c. becomes, after long ſtanding, but eſpecially when expoſed to the ſun, *depurated*, ſo as at length to become abſolutely dephlogiſticated; and that this air being continually emitted by all water, expoſed to the action of the ſun's rays, muſt contribute to the melioration of the ſtate of the atmoſphere in general.

When I have kept water a long time in the ſhade, it has not generally yielded any other kind of air than it would have yielded at the firſt; and, though, when it has been kept in an open veſſel, the air has been better, it has never been ſo good as the air in the ſame kind of water that has been expoſed a much leſs time to the ſun.

No degree of *warmth* will ſupply the place of the ſun's light; and though, when the water is once prepared by expoſure to the ſun, warmth will ſuffice to *expel* that air; yet, in this caſe, the air has never been ſo pure, as that which has been yielded ſpontaneouſly, without additional heat. The reaſon of this may be that, beſides the air already depurated, and on that account ready to quit its union with the water, heat

heat expels, together with it, the air that was phlogiſticated, and held in a cloſer union with the water; which air the action of light, whatever that be, would in time have depurated alſo.

The quantity of air yielded by water ſpontaneouſly far exceeds that which can be expelled from it by heat. Indeed I have frequently obſerved that whatever circumſtance depraves air, leſſens alſo the quantity of it; ſince it requires a large quantity of dephlogiſticated air to make a ſmall quantity of phlogiſticated air, or even common air, which is air partially phlogiſticated.

If the water naturally contains fixed air, yet in conſequence of this expoſure to the light it is all diſſipated, and the natural reſiduum of it becomes pure dephlogiſticated air. For no fixed air at all, but only the pureſt dephlogiſticated air, is at length procured from it; and water impregnated with fixed air yields, after this expoſure, the greateſt quantity of dephlogiſticated air.

I ſhall not recite the particular experiments that have led me to theſe concluſions;

ſions; partly becauſe they are too numerous, and alſo becauſe I hope to repeat them to more advantage in a more favourable ſeaſon of the year; but I ſhall ſelect a few, which ſufficiently eſtabliſh every thing that is of importance in theſe concluſions.

The large receiver of which I have made mention, as having ſerved me for a magazine of air, and which I find contains 135 ounces of water, I have already obſerved yielded, when filled with pump water, nine ounce meaſures of very pure dephlogiſticated air, after being expoſed to the ſun from the 17th of September to the 14th of October. I then filled it with freſh pump water, and placed it in my laboratory till the 8th of December (with its mouth inverted, as before, in a diſh of the ſame water) and in all this time not a ſingle bubble of air came from it. But on being placed in a ſouth window it immediately began to yield air, and continued ſo to do whenever the ſun ſhone, till the 21ſt of January following, when there might be about four ounce meaſures of air.

I then

I then placed the receiver, and the diſh in which it ſtood, in a large pan of water, which I made to boil, and kept boiling the whole day, till no more air could be diſcharged from it; and the next morning, when it was cold, I examined the air, and found it to be in all ſix ounce meaſures. No part of it was fixed air, and one meaſure of it and two of nitrous air occupied the ſpace of .9 meaſures; whereas, with the air produced ſpontaneouſly, in the light of the ſun, the ſame meaſures were .44, and the quantity nine ounce meaſures.

Beſides, I am by no means certain that, in the former caſe, the water had yielded all the air that it would have done. For at that time I imagined it was the green matter that yielded the air, and therefore thought it to be of no conſequence, with reſpect to what I then had in view, whether I got little or more of it.

Having, by the preceding experiments, aſcertained, in ſome meaſure, the quantity and quality of the air yielded by this water, both wholly, and alſo in part ſpontaneouſly

neouſly, I filled the ſame receiver with the pump water; and, without expoſing it to the light at all, put it into the pan of water, and found that, after keeping it boiling all day, I got no more than 1$\frac{1}{4}$ ounce meaſures of air from it. Examining it the next morning, no part of it was fixed air, and one meaſure of it and one of nitrous air occupied the ſpace of 1.26 meaſures; when, with the common air, the meaſures of the teſt were 1.3; ſo that it was a little better than common air.

When I again expelled air from the ſame pump water, and examined it *immediately*, I found a part of it to be fixed air; but I am confident not ſo great a proportion of it as I had ſometimes before found in the ſame pump water. It may, therefore, be worth while to examine the air from the ſame water at different times of the year, and in other different circumſtances.

In my former publication, Vol. III. p. 267, I have obſerved that when water has been made to imbibe inflammable, or nitrous air, the air that is immediately after-

afterwards expelled from it is alſo inflammable or nitrous; though whether they were ſo in exactly the ſame degree I cannot pretend to ſay. However, now, on obſerving the operation of water on the air contained in it, and retained a conſiderable time, I boiled a quantity of water, and then made one portion of it imbibe common air, another phlogiſticated air, and a third dephlogiſticated air. In this ſituation they remained near a fortnight, but not expoſed to much light; when the common air that was not abſorbed by the water I found to be conſiderably phlogiſticated, which agrees with my former obſervation on the ſubject; and the air expelled from the water was much purer than the common air. For one meaſure of it and one of nitrous air occupied the ſpace of one meaſure. The dephlogiſticated air alſo came out of the water a little improved; but the phlogiſticated air was not ſenſibly mended.

Had theſe kinds of air continued longer in the water, and been expoſed to the light of the ſun, it is probable that the common air would have been ſtill more pure, and alſo that even the phlogiſticated air would have been mended. I ſhall not fail to make a full trial of this whenever I reſume the experiments.

Till theſe laſt experiments on air in water, I had concluded that the air naturally contained in water is always mixed with fixed air, and worſe than common air; and I had not at all conſidered the alteration that *length of time*, or any other circumſtances, as expoſure to the *air*, *light*, &c. might make in it. Theſe circumſtances, however, and, as I think, more eſpecially the laſt, make a moſt eſſential difference in the caſe, and ſhould be particularly attended to when an account is taken of the air that any kind of water naturally yields. I am confident, from the experiments I have made at different times on my own pump water, hinted at above, that the air contained in it is in different ſtates at different times. I am alſo ſatisfied that the ſame is true with reſpect to the water of the Hot-well at Briſtol.

Having myſelf examined the air contained in the Bath water, as will be ſeen Vol. I. p. 222, I was

I was willing to make the ſame experiment on the water of Briſtol hot-well, and did go to Briſtol partly with a view to it; but finding I had not ſufficient time, I deſired Mr. Becket, on whoſe ſkill and care I could intirely depend, to make the experiment for me. He was ſo obliging as to undertake it, and he ſent me the ſatisfactory account contained in his letter to me, which will be found in the *Appendix* to this volume, where it will be ſeen that the air expelled from this water was better than common air.

This being the firſt fact that I had met with of the kind, I requeſted that he would ſend me a quantity of the water taken freſh from the ſpring, and then bottled, and carefully ſealed up immediately. He did ſo, and the moment it came to my hands I opened one of the bottles, and ſlipping into it another cork, provided with a glaſs tube properly bent for the purpoſe, and plunging the whole in a pan of water, I made it boil; and expelling all the air I could from it by this means, I found it rather worſe than common air, but to contain no fixed air.

I then expoſed a quantity of it in a phial without a cork in a South window, and two months after I examined it, together with another quantity of the ſame water, which had been kept corked in the ſhade; when I found the air in the former ſo pure that one meaſure of it and one of nitrous air occupied the ſpace of one meaſure; whereas the air from the latter, which had been kept corked, and in the ſhade, was both leſs in quantity, and worſe in quality, being juſt what I had firſt found it to be, viz. worſe than common air; but, as then, without any mixture of fixed air. This experiment is another confirmation of the influence of expoſure to the *air*, if not to the *light*, on air contained in water.

Being now fully ſatisfied that air is purified by being retained in water expoſed to the air and light, I regretted exceedingly that I had not made the obſervation before I was at Lymington, that I might have made a trial of the air contained in ſea water, in the heat of the late ſummer. I immediately, however, wrote to ſuch of my friends as either had opportunities of making the proper ex-

periments

periments themſelves, or of procuring them to be made by others, and particularly to my friend, Dr. Percival, whoſe zeal to promote all ſcientifical purſuits is ſufficiently known, requeſting that he would aſk the favour of Dr. Dobſon, or ſome other friend at Liverpool. The Doctor was ſo obliging as to go through the examination without delay, and with every requiſite precaution, as will be ſeen by his own letter in the *Appendix*; and he then found the air of ſea water to be better than common air.

I wiſh the experiment may be made at more places, and at different times of the year. I have little doubt, however, but that there will be ſufficient reaſon to conclude, that air being imbibed by water, and eſpecially by waters of ſo immenſe an extent, and ſuch full expoſure, as thoſe of the ſea, and again emitted, comes out *depurated*, and free from the principle with which it was charged by animal reſpiration, putrefactive proceſſes, ignition of inflammable ſubſtances, &c. and therefore that the obſervation will be of conſiderable value. Perhaps it will be imbibed by water in winter, and emitted in ſummer, though the temperature of the ſea does not vary ſo much as that of bodies of water of leſs depth and magnitude.

At the time of my firſt publication on the ſubject of air, I concluded that it was "not "improbable, but that the agitation of the "ſea, and of large lakes, might be of ſome "uſe for the purification of the atmoſphere, "and that the putrid matter contained in "water might be imbibed by aquatic plants, "or be depoſited in ſome other manner." Vol. I. p. 98. This was advanced in conſequence of my having found all kinds of noxious air to be depurated to a certain degree, ſo as to be rendered fit for reſpiration, by agitation in water; but I had no idea at that time of the effect of water on air being ſo great as I now find it to be. Indeed, I then attributed this effect to the mere *contact* of the air with the water, and not to its being properly abſorbed by the water, and for a time *incorporated* with it.

It will

It will probably be imagined that the refult of the experiments recited in this fection, throws fome uncertainty on the refult of thofe recited in this volume, from which I have concluded that air is meliorated by the *vegetation* of plants, efpecially as the water by which they were confined was expofed to the open air, and the fun in a garden. To this I can only fay, that I was not then aware of the effect of thefe circumftances, and that I have reprefented the naked *facts*, as I obferved them; and having no great attachment to any particular *hypothefis*, I am very willing that my reader fhould draw his own conclufions for himfelf.

I muft inform him, however, that my experiments at Leeds were made in a North window of the houfe, where the influence of the light on the water could not be very confiderable, that fome of the proceffes were completed in two days, and generally in about a week; and that the water within the jars was fo fmall, in proportion to the quantity of air, that I do not at prefent imagine that the melioration of air at that time could have been owing to it. Besides, as I have observed, I frequently kept air in the same exposure, with respect to *water*, *light*, and every other circumstance that occurred to me to attend to, and the same space of time, but without any plant vegetating in it, when there was no sensible melioration of it.

SECTION XXXIV.

Of Inflammable Air.

1. *Of the Production of Inflammable Air from Iron and a Solution of Galls.*

INFLAMMABLE air was procured by Mr. Cavendish from iron, zinc, and tin, by the vitriolic and marine acids. At the time of my former publications I had procured it from copper and lead by the marine acid, from a great variety of animal, vegetable, and mineral substances containing phlogiston, when dissolved in marine acid air, from some of the metals dissolved in the vegetable acid, and also from several of them by *mere heat* with a burning lens, or in a common

fire.

fire. I now frequently find it an inconvenient confequence of heating things in a gun barrel. For fome inflammable air, difcharged from the iron, mixes with the air that I am procuring, in fuch a manner, as to make the refult of the procefs a little uncertain; fo that, in all experiments that require much accuracy, I ufe fmall glafs retorts, or glafs tubes.

Since my laft publication I have procured inflammable air, in a confiderable quantity, by diffolving iron filings in a folution of galls; and very probably the fame would be produced by means of any other aftringent fubftance. Indeed moft things that really *decompofe* the metal, and do not unite with the *whole mafs* of it, will, I imagine, fet loofe the phlogifton it contains, in the form of inflammable air; though, in feveral of the cafes, the phlogifton might join fome of the principles in the menftruum, and contribute to compofe a different fubftance.

I was led to this obfervation of the production of inflammable air by the folution of galls, in confequence of being informed by Mr. Delaval, that ink might be made by putting iron to the ſolution of galls; for that the acid in the vitriol, which is commonly uſed for the purpoſe of making ink, is an unneceſſary, and frequently an inconvenient ingredient.

Having mixed a quantity of pounded galls, iron filings, and water, I firſt obſerved, that, after a day or two, the whole maſs was very much ſwelled, and that it was full of bubbles of air, which at the ſurface were very large. Suſpecting, from the ſmell, and other circumſtances, that the air contained in them was inflammable, I burſt ſeveral of them near the flame of a candle, and found that they all made ſmall exploſions, ſo that I could have no doubt concerning the quality of the air.

I then mixed three ounces of pounded galls with water and iron filings, the quantity of which I did not note; and covering them with a large jar full of water, found that, in about a week, they had produced ſix ounce meaſures of air, which was ſtrongly inflammable, exactly like that which is produced

produced from iron by the acids. In the ſame manner I procured a quantity of this inflammable air by putting the above-mentioned mixture into a phial with a ground ſtopper and tube. But this proceſs is too ſlow for any uſe.

2. *Inflammable Air from Oil of Turpentine.*

I have mentioned the property of oil of turpentine to abſorb air, Vol. III. p. 112, and found that, in its natural ſtate, it contains a conſiderable quantity; but it did not occur to me at that time to examine the nature of the air it contained. Intending to proſecute the ſubject a little farther, I lately opened a pint phial, half filled with this kind of oil, and the cork being very tight, there ruſhed out of it a great quantity of air, when applying the flame of a candle to the mouth of the phial, I found the remainder to be ſtrongly inflammable. The oil was then quite full of air bubbles, and by the heat of boiling water I expelled from a quantity of it an equal bulk of air, all ſtrongly inflammable, like that which is obtained from metals.

tals. It was eight or ten hours in giving this air. When I could perceive the colour of the flame, I found it to be blue.

I then took a quantity of the fame kind of oil, which had been kept in another phial, but I found the air incumbent upon it, within the phial, to be only common air; but making it boil in a retort, I expelled from it twice its bulk of air, all ftrongly inflammable. I could not diftinguifh the colour of its flame.

When I had thus expelled all the air which a quantity of this oil of turpentine feemed to contain, I agitated it very ftrongly, and frequently, in the courfe of two days, in order to make it imbibe more air, that I might expel it again; but I did not find that it had imbibed more than a very fmall quantity, and this, when it came out again, was only common air flightly phlogifticated. The firft boiling had made it brown, and very vifcid.

3. *Whether there be any Acid in Inflammable Air.*

In my firft publication on the fubject of air, I had concluded, that it confifted of acid

acid and phlogiſton, together with ſome earth. Afterwards, having got the ſame air without any acid, by heat only, I was ſatisfied that the acid was not a neceſſary ingredient in its compoſition; and I was farther confirmed in this opinion, by finding that, when I mixed inflammable and alkaline air together, they formed no ſort of union. For when the alkaline air was abſorbed by water, the inflammable air was left juſt as before.

This obſervation has been contradicted by Dr. Higgins, who pretends to have proved the preſence of an acid in inflammable air by a great cloudineſs that attends the mixing of it with alkaline air. This, I own, would be a deciſive proof of the alkaline vapour having met with ſomething acid in the inflammable air. But having repeated the experiment with the greateſt care, and with circumſtances which render my obſervation more accurate and deciſive than before, I am ſatisfied that Dr. Higgins, neglecting the precautions, that are neceſſary to be applied in theſe experiments, has made ſome miſtake.

I

I conjecture that, making the inflammable air with a pretty ſtrong acid, either the vitriolic, or marine, he produced a quantity of *acid air* together with the inflammable air; and, without ſeparating them by water, admitted the alkaline air to that mixture. It would then, of courſe, unite with the acid air, and form the cloud that he deſcribes. But if, after this, he had ſeparated the alkaline air by water, he would have found the inflammable air unchanged in its bulk or properties.

The manner in which I made the experiment, which I think proves deciſively that there is not *neceſſarily* any acid in inflammable air (at leaſt, none in a ſtate to admit of its being decompoſed by the mixture of alkaline air) was as follows. Having produced the inflammable air in the uſual manner, with a weak vitriolic acid, making the air to paſs through water, and keeping it in water ſome time, that the ſuperfluous acid (if there ſhould have been any in it) might be abſorbed, I transferred it, by means of a bladder, into a jar previouſly filled with quickſilver

quickſilver, and ſtanding inverted in a trough of quickſilver. The alkaline air was alſo contained in a jar ſtanding in the ſame trough. Then taking a meaſure of one, and the ſame quantity of the other, and mixing them in a jar previouſly filled with quickſilver, and, two of my friends being along with me, we obſerved that there was not the leaſt cloudineſs occaſioned by the mixture. When they were together they occupied exactly the ſame ſpace that they had done when they were ſeparate; and, afterwards, water being admitted to them, the alkaline air was abſorbed, and the inflammable air was left undiminiſhed; and, as far as I could judge, it was in all reſpects, the ſame that it had been before.

4. *Inflammable Air not affected by the Electric Spark.*

Inflammable air is the only kind of air that is not affected by the *electric ſpark* or exploſion. This I had obſerved before; but I have ſince made the experiment ſo as to to ſatisfy myſelf more fully with reſpect to it. I confined a very ſmall quantity of this air in a glaſs tube, the diameter of which was not much more than one tenth of an inch, and the length of the column of air did not exceed half an inch, and it was confined by water tinged with the juice of turnſole. In this ſmall quantity of air I took the electric ſpark half an hour, from the large and powerful machine mentioned on a former occaſion, without producing the leaſt ſenſible change in the dimenſions of the air, or in the colour of the liquor in which the ſpark was taken. I therefore think I may ſafely conclude, that inflammable air, at leaſt that which is produced from iron by oil of vitriol, is of ſuch a conſtitution, as to be incapable of being decompoſed by this proceſs.

5. *Inflammable Air decompoſed by Heat, in Tubes of Flint Glaſs.*

This kind of air alſo remains unchanged when it is expoſed to heat in a tall jar of flint glaſs, in which it had free liberty to expand

expand. I made this experiment at the ſame time with the ſimilar one that has been mentioned before on nitrous air. This air, as well as the nitrous, recovered its former dimenſions when it was cold, and appeared to be unchanged in its quality.

A very ſingular decompoſition of inflammable air I obſerved in conſequence of expoſing a great variety of ſubſtances to the influence of a ſand heat, which I kept up for ſeveral months. Among other things, I buried in this hot ſand glaſs tubes hermetically ſealed, and previouſly filled with all the different kinds of air. I filled them in the following manner.

Having provided myſelf with glaſs tubes about four feet long, and about one-third or one half of an inch in diameter, and of ſuch a thickneſs as that I could eaſily melt them with the flame of a couple of candles and a common blow pipe, I firſt ſealed the tubes at one end, then filled them with quickſilver, and placed them inverted in a baſon of the ſame. After this, either transferring the air in a bladder, from the jars in which they had been ftanding in water, or generating the air a frefh, if it was of a kind not to bear the contact of water, I filled the tubes completely with the kinds of air on which I wifhed to make the experiment, difplacing the quickfilver. This being done, I inclined the tube, and applying the flame of my candles with fome care (holding the blow pipe in my mouth only, and keeping firm hold of the tube on each fide of the place to which I was applying the heat) I melted the glafs, and took off what lengths of it I pleafed; and every piece was, of courfe, hermetically fealed. Thefe pieces I marked with a file, keeping an account of the meaning of the marks, that when I took them out of the fand, I might prefently know with what kind of air they had been filled.

When I was performing this part of the procefs with inflammable air in flint glafs tubes, I obferved that the places to which I applied the heat were generally tinged black; but I gave little attention to this circum-

circumſtance, thinking it might be ſomething accidental; and without any particular expectation, I buried theſe tubes in the ſand, together with the others. This was on the 25th of September 1777.

On the 20th of January following, I examined theſe tubes, together with every thing elſe that had been expoſed to the ſame heat. The tube containing the inflammable air was ten inches long, and by ſome accident was broke; but it was jet black throughout. At this I was very much ſurprized, but I did not then ſuſpect that it was at all owing to the inflammable air with which it had been filled; thinking it might have been occaſioned by ſome phlogiſtic matter in the ſand, or in ſome of the veſſels that had burſt in its neighbourhood.

Reflecting, however, on this odd circumſtance, and thinking, from the uniformity of the tinge, that, *poſſibly*, it might have been occaſioned by the inflammable air, I filled another ſmall glaſs tube with the ſame air; and, ſealing it hermetically,

 buried

buried it deep in ſand, contained in an iron pot, which I ſet on the fire, and made very hot, nearly red; and taking it out the next day, I found the tube quite black, except a ſmall part on one ſide of that end which had been uppermoſt, about two inches higher than the other, and conſequently had not been expoſed to ſo great a degree of heat.

Being now fully ſatisfied that the blackneſs of the tube was *certainly* occaſioned by heating the inflammable air within it, in circumſtances in which it could not expand, I proceeded to examine the ſtate of the air. But, in the firſt place, to aſſure myſelf there had been no communication between that air and the external air, by means of ſome unperceived crack in the glaſs, I plunged it in water, and exhauſting the air over it, did not perceive that any bubble eſcaped. Then breaking the end of the tube under water I examined it and found it not to be inflammable. Sometimes, however, when I have only made the tube juſt black throughout, by applying the flame of a candle,

candle, with a blow pipe, to every part of it, in ſucceſſion, the air has ſtill been inflammable.

Putting two glaſs tubes, about four inches in length, and a quarter of an inch in diameter, into a ſand furnace, I kept them in it two days; when I took them out, and obſerved that the tube which I had placed at the bottom of the ſand, in the greateſt degree of heat, was nearly melted, and perfectly *blue*, like indigo; while the other tube, which had not been expoſed to ſo great a degree of heat, was of a beautiful jet black throughout.

Examining the air in theſe tubes, I found that in the black tube reduced to one third of its bulk, and mere phlogiſticated air. It did not make lime water turbid, was not affected by nitrous air, and was not inflammable. The air in the blue tube, or that which had been expoſed to the greateſt degree of heat, was reduced to the quantity of a very ſmall bubble, ſo that no experiment could be made upon it. I have no doubt, however, that it was phlogiſticated.

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At one time I had a ſuſpicion that this blackneſs communicated to the glaſs was ſomething precipitated from the iron, by the ſolution of which the inflammable air had been made; but I was ſoon convinced of the contrary, by finding that the effect was the very ſame when the inflammable air was made from *zinc*.

I ſoon found that there was no occaſion for ſo long a proceſs to produce this effect, at leaſt upon the glaſs. For it began to be diſcoloured the moment it was red hot, or rather when it became ſoft; as was evident by holding one of the tubes in an open fire, or in the flame of a candle. For wherever the heat was applied, the blackneſs immediately took place, without affecting any other part of the tube.

When I examined this black tinge narrowly, I found that it did not penetrate the glaſs; but formed a delicate ſuperficial tinge, leaving the glaſs as perfectly poliſhed as before the proceſs. But the blackneſs was indelible: at leaſt it could not be ſcraped off without tearing the ſurface of the glaſs, and

and it made no change in it with reſpect to electricity. For the tube thus blackened was as perfect a non-conductor as ever.

The blue colour of the glaſs that was moſt heated, Mr. Delaval informed me, was owing to ſomething of *iron* in the compoſition of the glaſs. That it alſo depended upon the *degree of heat*, I aſcertained by placing one of theſe tubes in a vertical poſition in the ſand heat. For the lower end of the tube, which was moſt heated, had acquired a deep blue colour, and it paſſed into the black at the upper end of the tube without any intermediate colour. There was alſo no other colour higher than the black; ſo that the firſt tinge that the glaſs receives is a perfect black. Yet viewing the firſt tinge that it receives by the light of a candle placed beyond it, it ſeemed to have a ſhade of *red*.

As I was ſenſible that the blackneſs was owing to the precipitation of *phlogiſton* from the inflammable air, I thought it poſſible that ſome ſubſtance which had a near affinity with phlogiſton might diſcharge it; and trying *minium*, it ſucceeded immediately. Having filled one of theſe black tubes with this metalic calx, the moment I made it red hot, the blackneſs intirely diſappeared, and left the tube as tranſparent as ever it had been.

In the firſt experiment of this kind I uſed minium out of which all its air had been expelled by heat, and which is of a yellow colour. In this proceſs it became whiter, and adhered a little to the glaſs. When I ſcraped it off, I could not be quite ſure that any part of it was become real *lead*; but it evidently approached towards a metalic ſtate, by being of a more compact texture than before.

In this ſtate of the experiments I communicated the reſult of my obſervations to my friend Mr. Bewly, who ſuggeſted to me, that, probably, it was the *lead* in the glaſs tubes that had attracted the phlogiſton; and I preſently found this to be the caſe. For when I had filled a *green glaſs* tube with the inflammable air, and ſealed it hermetically, as I had done the flint

flint glaſs tubes, I expoſed it to a melting heat, which is greater than that which flint glaſs will bear, without producing any change of colour in it. What remained of the air in the tube, that did not eſcape when part of it was melted, was ſtill ſtrongly inflammable.

It appears, therefore, from this experiment, that the calx of lead, in the form of glaſs, has a ſtronger affinity with phlogiſton than any thing in the compoſition of inflammable air, in a degree of heat capable of melting glaſs. Or, if there be no proper conſtituent part of inflammable air beſides phlogiſton, the attraction of the calx is ſo great, as to reduce the phlogiſton from an elaſtic and uncombined ſtate to a fixed and combined one.

Having, by means of theſe glaſs tubes, effected a complete decompoſition of inflammable air, the phlogiſton in it having united with the glaſs of the lead; I thought that, if there had been any *acid* in its compoſition, it would then be diſengaged, and be found in the tube. In order to find whether

ther there was any acid in it, or not, I poured into one of theſe tubes a ſmall quantity of water made blue with the juice of turnſole; but it came out as blue as it went in.

5. *Inflammable Air diminiſhed by Charcoal.*

In purſuance of the Abbé Fontana's experiment on the abſorption of air by charcoal, I dipped pieces of hot charcoal into a phial of inflammable air, and immediately inverted it in quickſilver. When one third of the whole quantity was imbibed, I found that both the remainder, and that which was again expelled from the charcoal, by plunging it in water, were inflammable; the former not to be diſtinguiſhed from what it had been, but the latter a little leſs inflammable.

6. *Whether inflammable or nitrous Air contain more Phlogiſton.*

It is well known that both nitrous and inflammable air contain phlogiſton, but in very *different ſtates*, becauſe their ſpecific gravities, and

and other properties, are moſt remarkably different. Many ſchemes have occurred to me to aſcertain the proportion of phlogiſton that each of them contains; and at length I thought of attempting the ſolution of this problem by the help of that ingenious experiment of Mr. Warltire's, mentioned in the Appendix to my third volume p. 367, *viz.* burning inflammable air in a given quantity of common air. For though inflammable air will not part with its phlogiſton to common air when *cold*, it will, like other combuſtible ſubſtances, when heated to a certain degree. It is then decompoſed, and the phlogiſton that entered into its compoſition phlogiſticates the air in which it is burned; and the degree of phlogiſtication may be meaſured by the teſt of nitrous air. I, therefore, proceeded as follows.

In an eight ounce phial, containing many nails, and a quantity of water with oil of vitriol, I produced inflammable air; and making it burn with a ſmall flame, at the orifice of a glaſs tube through which the

the air was tranſmitted (being cemented into the cork of the phial) I covered the flame with a receiver that contained twenty-one ounce meaſures of air, ſtanding in water. After ſix minutes, the flame went out; when, immediately catching the air that was produced in the next ſix minutes, and alſo in the ſix minutes following, I concluded that ſeven ounce meaſures had been produced, and decompoſed, during the ſix minutes in which it had continued to burn.

Then examining the air in which it had burned, I found it ſo far phlogiſticated, that equal meaſures of it and of nitrous air occupied the ſpace of 1.65 meaſures; and common air mixed with one third as much nitrous air, being again mixed in equal proportions with the ſame freſh nitrous air, occupied the ſpace of 1.68 meaſures. It appeared, therefore, that the 21 ounce meaſures of air, having received the phlogiſticated of one third as much inflammable air, *viz.* ſeven ounce meaſures, was about as much phlogiſticated as it would have been with a mixture of the ſame proportion of nitrous air. Conſequent-

ly,

ly, equal meaſures of nitrous and inflammable air contain about equal quantities of phlogiſton.

Of this curious problem, however, I have obtained a more accurate ſolution from the mode of experimenting introduced by that excellent philoſopher Mr. Volta; who fires inflammable air in common air, by the electric ſpark, and conſequently can determine the exact proportion of the inflammable air decompoſed in a given quantity of common air. The reſult of this proceſs agreeing with that of the former, leaves little doubt with reſpect to the concluſion I have drawn from them.

Having prepared a ſtrong glaſs tube, in one end of which I had cemented a piece of wire, I filled it with water, and introduced into it another piece of wire, ſo as to come within about half an inch of the former wire, that an electric exploſion might eaſily paſs between them.

Into this tube, thus prepared, I transferred, in the firſt place, one meaſure of inflammable air, and three of common air; and then,

then, by means of an electric exploſion between the wires, in the central place of the air, I fired all the inflammable air, which would then be decompoſed, and, of courſe, part with its phlogiſton to the common air with which it was mixed. After the exploſion, I accordingly found it to be completely phlogiſticated. This alſo would have been the conſequence of mixing the ſame proportion of nitrous air with the common air. But to determine the problem with accuracy, it was neceſſary to uſe ſuch a proportion of inflammable as would only phlogiſticate the common air in part.

I therefore mixed one meaſure of inflammable air with *three* meaſures of common air, and after the exploſion found it be ſo far phlogiſticated, that one meaſure of this and one of nitrous air occupied the ſpace of 1.8 meaſures; and this I alſo found, by the ſame teſt, to be exactly the ſtate to which a mixture of one meaſure of the ſame nitrous air brought three meaſures of the ſame common air.

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In order to obtain a farther confirmation of my conclusion, I mixed one measure of inflammable air with *four* measures of common air; and after the explosion I also found, by the test of nitrous air, that it was phlogisticated exactly as much as by the mixture of an equal quantity of nitrous air. And repeating the experiment with the same proportion of inflammable and common air, I found that after the explosion the air was diminished, without mixing with nitrous air, just as much as one measure of nitrous air diminished four measures of common air, *viz.* from 7.4 to 5.2 measures.

SECTION XXXV.

Of FIXED AIR.

1. *Of the Generation of Fixed Air from the Vitriolic Acid.*

AT the time of my laſt publication I think I had clearly aſcertained the generation of fixed air from *ſpirit of nitre*, and various other ſubſtances, which have never been ſuſpected to contain it, ſuch as ſpirit of wine, &c. I have now as evident a proof of the generation of fixed air from the *vitriolic acid* united with ſpirit of wine, or with ether, which is produced from them both; ſo that theſe two acids, *viz.* the vitriolic and nitrous, agree in being capable of forming both dephlogiſticated and fixed air; a circumſtance which may throw conſiderable light on the conſtitution of theſe acids, and the relation they bear to each other.

After going through the proceſs for making ether, from concentrated oil of vitriol

vitriol and rectified ſpirit of wine, I had the curioſity to puſh the proceſs as far as it would go, in order to examine whether any kind of air would be yielded in any ſtage of it. I therefore continued the diſtillation till the whole reſiduum was converted into a black maſs, full of groſs matter; and taking as much of the black lumps as filled about one-fifth of an ounce meaſure, I put them into a tall glaſs veſſel, and diſtilled them to dryneſs in a red hot ſand heat.

The firſt air that came over was the common air a little phlogiſticated, then the vapour of the watery part, and after that a large quantity of air at firſt clear, but towards the middle of the proceſs very turbid and white, but clear again at the laſt. I received in all about a pint and a half, in four portions, each of which contained about four-fifths of fixed air, and the reſt inflammable, burning with a blue flame; but the proportion of fixed air was ſomething greater in the middle portions than either in the firſt or the laſt. I thought it poſſible that the cork, with which, as well as with clay and ſand, the glaſs tube was joined to the glaſs veſſel that contained the materials, might ſupply the inflammable air in part, as I perceived it was corroded and become black. It may be worth while to repeat this proceſs in a glaſs retort.

Having gone over this proceſs with ſpirit of wine, I recollected the black matter that was produced when I got vitriolic acid air from vitriolic acid and ether; and therefore determined to repeat that proceſs and carry it farther; to ſee whether I ſhould, in any part of it, get fixed air, as in the preceeding experiment with the ſpirit of wine.

I therefore put one-eight part of vitriolic ether to a quantity of freſh diſtilled oil of vitriol, and in a glaſs phial with a ground ſtopper and tube, and with the heat of a candle, I got from it a great quantity of air, part of which was vitriolic acid air, which was abſorbed by the water, but I obſerved, as the proceſs advanced, the part that

that was not readily abſorbed by water kept increaſing, till at length the greater part of the produce was of this kind; and in the middle of the proceſs it was very turbid. Examining this air it appeared to be fixed air, making lime water turbid, and being readily abſorbed by water; but there was a reſiduum of phlogiſticated air, about one-ſixth of the whole.

I then put the remaining materials, which were about an ounce meaſure, into a glaſs veſſel; and with a ſand heat I collected much more air than before, about two pints in all, the firſt part of which was the pureſt fixed air I had ever ſeen, having the ſmalleſt reſiduum. The laſt portion had more reſiduum, and this burned with a lambent blue flame. But this inflammable matter might poſſibly come from the cork with which the veſſel was cloſed, as before; though I think it not ſo probable. At laſt the proceſs was interrupted by an accident; but I concluded, from ſeveral circumſtances, eſpecially from the time that elapſed before the vapour ceaſed to iſſue from the orifice of the veſſel (which continued buried in the hot ſand) that more than twice the quantity of air might have been collected. The air had been very cloudy before the laſt portion, which contained the reſiduum of inflammable air.

From this experiment, eſpecially that with the ether, in the glaſs phial and ground ſtopper, I think it is pretty evident, that fixed air is a *factitious ſubſtance*, and that the vitriolic, as well as the nitrous acid, may be converted into it.

2. *Of Fixed Air imbibed from the Atmoſphere.*

From a ſolution of quickſilver in the nitrous acid, which had ſtood expoſed to the air a conſiderable time, I once got a conſiderable quantity of fixed air, together with that which was dephlogiſticated (See Vol. III. p. 352) whereas I never got any fixed air when I made the diſtillation immediately after the ſolution. It was moſt probable, therefore, that the fixed air had been

been attracted from the atmofphere. However, as it was *poffible* that this production of fixed air might have come from the mixture itfelf, in a courfe of time; efpecially as I had found that, in fome cafes, fixed air was produced either from, or by means of, the nitrous acid, in the decompofition of fubftances that did not contain it; I made a folution of mercury in ftrong nitrous acid, and kept it in a phial with a ground ftopper from May 1776 to the 12th of September following. Pouring this folution as quickly as poffible into a fmall long necked retort, I got from it, in a fand heat, a large quantity of air, firft nitrous, and then dephlogifticated, but no part of it was fixed air, not making lime water in the fmalleft degree turbid. It was in this procefs that I got that exceedingly pure dephlogifticated and nitrous air mentioned before; but I do not fuppofe that the peculiar purity of it was at all owing to the *length of time* that the folution was kept before the diftillation.

In Vol. III. p. 313, I have recited inſtances of wood aſhes imbibing fixed air from the atmoſphere. To be more fully ſatisfied with reſpect to it, and alſo the quantity of fixed air imbibed by them in a given time, I kept the ſame aſhes, and extracted air from them at certain intervals. I alſo did the ſame thing with ſeveral other ſubſtances of a ſimilar nature, and the reſults were as follows.

On the 18th of April 1778, I extracted all the air I could from half an ounce of wood aſhes, and got about eighty ounce meaſures, half fixed air, and half inflammable throughout; and on the 25th of the ſame month I repeated the proceſs on the ſame aſhes, in a gun-barrel, and got from them twenty ounce meaſures of air, the greateſt part of which was fixed air, and the reſt inflammable. The aſhes were become almoſt black after the experiment. At firſt I imagined it might be the charcoal revivified by the phlogiſton from the gun-barrel; but I afterwards found it to be a kind of glaſs, or ſlag, the heat having been

been ſo great, as to vitrify the aſhes; and the phlogiſton from the iron had given them the black colour.

June the 2d, I extracted, by heat, in a gun-barrel, from wood aſhes from which air had often been extracted before, in the ſame manner, and the laſt time on the 9th of May preceding, all the air that they would yield. It was twenty-one ounce meaſures; the firſt portions of which were half fixed air, and afterwards one-third; the remainder in both caſes being inflammable, probably from the iron. A good deal of moiſture diſtilled from theſe aſhes, though they ſeemed to be perfectly dry. After the proceſs, they weighed 18 dwts. and, judging from their colour, not much more than two-thirds of them had been affected by the heat.

On the 23d of October following, the ſame wood aſhes weighed 19 dwts. 12 grs. and I got from them, in a gun-barrel, about thirty ounce meaſures of air, of which more than 25 ounce meaſures was pure fixed air, the remainder inflammable, burning with a blue flame. They had not all been equally affected by the heat. After the procefs, they weighed 18 dwt. 6 gr. That they had attracted fixed air is evident, efpecially from the laft procefs, in which the greateft part of it was very pure.

On the 18th of April 1778, I got, from an ounce of *pit-coal afhes*, in a gun barrel, nineteen ounce meafures of air, of which at firft two thirds, and at the laft one third was fixed air, and the reft inflammable. On the 24th of the fame month, I extracted from the fame pit-coal afhes (which, as well as the wood afhes in the preceding experiment, had been expofed to the open air in a difh, fo as to lay about half an inch thick) 110 ounce meafures of air; but with more heat than before. Of the firft part of this air one third was fixed air, but of the laft hardly any, the remainder being inflammable, burning with a blue flame; but fo faintly, that probably the greateft part of it was phlogifticated air.

Heating the fame afhes over again, in a fhallow iron veffel, and letting them cool, I got from them, by the fame procefs, fifteen ounce

ounce meaſures of air, one third of which was fixed air, and the reſt inflammable. But I obſerved, that when the aſhes came out of the gun barrel, they had the appearance of charcoal, but upon farther examination, I found it to be glaſs or ſlag; theſe aſhes having been vitrified by the heat, and having received phlogiſton from the iron, as in the preceding proceſs with wood aſhes. But theſe aſhes from pit-coal are vitrified with much leſs heat than the wood aſhes.

Common pit-coal, I have obſerved, yields no fixed air, though the *aſhes* do; but I have found that one ſpecies of pit-coal, called *Bovey coal*, yields fixed air in the firſt inſtance, which ſeems to indicate that there is ſomething of a vegetable nature in that coal. From half an ounce of this coal I got, in a gun barrel, about an hundred ounce meaſures of air, three fourths of which was fixed air throughout, and the remainder inflammable; the firſt part of it burning with a bright white flame, like inflammable air from common pit-coal, the laſt part exploding like inflammable air from metals,

only

only more faintly. Part of this air had probably come from the gun barrel.

I alfo found that *manganefe*, which had been calcined on the 10th of Nov. 1777, and again on the 15th of April 1778, yielded a fmall quantity of fixed air on the 2d of June following. From 1 oz. 18 dwt. of manganefe, which had been kept in a red heat a long time I expelled twenty ounce meafures of air, all fixed air. This was in a gun barrel, with as much heat as I could give to it, perhaps more than I had applied before.

The preceding experiments, relating to the imbibing of fixed air from the atmofphere, were made with vegetable and mineral fubftances. I made fome obfervations of the fame kind on animal fubftances. On the 24th of Feb. 1777, I took 1 $\frac{1}{2}$ oz. of *bone afhes*; and, in a gun barrel, I got from them a confiderable quantity of air, half fixed, and half inflammable. I then put fpirit of nitre to them, and obferved that the mixture was attended with great heat, and the emiffion of red vapours, and when dry they weighed

4 2 oz.

2 oz. 4 dwts. From half of this quantity I expelled about a pint and a half of air, one-fourth fixed, and the reſt dephlogiſticated. There remained little leſs than the original quantity of aſhes.

From the ſame bone aſhes, which had been moiſtened with ſpirit of nitre, I expelled, on the 15th of April 1778, about ten ounce meaſures of air, about one third of which was fixed air, and the remainder phlogiſticated. Theſe aſhes had been kept partly in an open diſh, and partly in a phial cloſe ſtopped, owing to my removing from one place to another, and not having an opportunity of making the experiments that I intended. On the 2d of June following, I extracted from the ſame bone aſhes five ounce meaſures of pure fixed air, the ſmall reſiduum being phlogiſticated. They then weighed 1 oz. 8 dwt. 6 gr.

From an ounce of the bone aſhes, from which air had been expelled on the 24th of Feb. 1777, but not thoſe on which the preceding experiment was made, I got by heat, in a gun barrel, on the 15th of April 1778, about

about fifteen ounce meaſures of air, almoſt pure fixed air. Theſe aſhes had been kept part of the time in a phial, and partly in an open diſh, as thoſe mentioned above.

From the ſame bone aſhes I was not able, on the 2d of June, to extract any air at all, nor again on the 23d of Oct. following.

It is evident from theſe experiments, that theſe bone aſhes (and the ſame is probably the caſe with the aſhes of other *animal ſubſtances*) have not the ſame property of drawing fixed air from the atmoſphere that the aſhes of vegetable and mineral ſubſtances have; but that the addition of ſpirit of nitre gives them that property. This obſervation may poſſibly be of ſome uſe in our inquiry into the nature of animalization.

3. *Attempts to extract fixed Air from various Subſtances.*

To the ſubſtances from which I had endeavoured, at different times, to extract air by heat, it may be juſt worth while to mention *crude antimony*. From one ounce of it, in a glaſs veſſel, and with a red ſand heat, I got

got very little air, not more than its bulk. The laſt portion was in a great meaſure fixed air, and the reſiduum extinguiſhed a candle. The antimony on which this experiment was made, and which had been pounded, formed a concrete maſs when taken from the fire.

I have before obſerved, that with much heat I got a little fixed air from *pipe clay*. I thought it was poſſible, that when it was mixed with the vitriolic or marine acid, it might yield more. I therefore tried both, but got no more air than about the ſame quantity as before.

From a quantity of *fluor*, in a gun barrel, I got a ſmall quantity of fixed air, the reſiduum being phlogiſticated, and at the laſt inflammable, from the gun barrel.

I expected to have got ſome air from *borax*, and for this purpoſe gave it as much heat as a green glaſs retort could bear; but I got little or nothing more than the common air in the retort, though I continued the proceſs till the glaſs melted.

4. *Fixed Air expoſed to Heat.*

I expoſed fixed air, as well as all the other kinds of air, to a continued heat, and in this caſe I made uſe of a green glaſs tube. I kept it in hot ſand a whole day, ſo hot that one end of the tube was much dilated, but had not burſt. Opening it under water, one half of the tube was inſtantly filled, and the remainder was the pureſt fixed air. I did not perceive any thing depoſited on the glaſs, as in the caſe of the marine and vitriolic acid air.

5. *Air from Charcoal and Precipitate per ſe.*

Many perſons, I find, have confounded phlogiſticated with fixed air, having concluded the whole of a quantity of air to be of the latter kind, though by far the greateſt part of it was of the former. This I found to be the caſe with reſpect to the air iſſuing from the ground in the Bath ſpring. Vol. II. p. 224. I alſo obſerve a miſtake of the ſame kind made by Mr. Lavoiſier; which my friends think it may be of ſome conſequence

to

to correct; he having inferred from it, that "common air is changed into fixed by the "addition of phlogiſton." See Rezier's Journal, Vol. V. p. 432.

He mixed an ounce of precipitate per ſe with 48 grains of charcoal, and then got from it air which had the five following properties. 1. It combined with water, and made it acidulous. 2. It was fatal to animals. 3. It extinguiſhed a candle. 4. It precipitated lime in lime water. 5. It united with alkalis, fixed and volatile, deſtroying their cauſticity. "Theſe properties," he adds, "are preciſely thoſe of that ſpecies of air "which is known by the name of fixed, or "mephitic air, ſuch as is obtained from all "metalic calces, with the addition of char-"coal, and ſuch as is diſengaged in fer-"mentation."

That ſuch a mixture of precipitate and charcoal would yield air which had the properties abovementioned, I had no doubt; but I was likewiſe well ſatisfied, from my experience in theſe matters, that the *whole*

produce

produce would not be fixed air, but contain a great proportion of other kinds of air, in fact, the very ſame that theſe materials would have yielded ſeparately; the dephlogiſticated air from the precipitate being depraved by the mixture of the other kinds of air.

However, for the ſatisfaction of my friends, who thought the experiment of conſequence, I mixed 1 dwt. of precipitate per ſe with half a pennyweight of well burned charcoal; and putting it into a green glaſs retort, expelled air from it, (but not all that it would have yielded) receiving the produce in different portions, when I found the firſt was three fourths fixed air, with the reſiduum inflammable. The ſecond was about as good as common air; and the third was phlogiſticated. All this, however, mixed together, would have exhibited the appearances (or very nearly ſuch) as Mr. Lavoiſier has deſcribed.

As to the concluſion of Mr. Lavoiſier's paper, in which he improperly ſtates it as my opinion, that fixed air is a compoſition of of common air and phlogifton. I have animadverted upon it in my fecond volume, p. 313.

SECTION XXXVI.

Experiments on Cream of Tartar.

TARTAR is a fubftance concerning which there has been a great deverfity of opinions among chemifts. On this account fome of my chemical friends requefted that I would examine what kind of air it yielded in different circumftances. Accordingly, to fatisfy them, and my own curiofity at the fame time, and without any particular expectation, for I had formed no opinion whatever with refpect to it, I began with putting a fmall quantity of the cream of tartar into fome oil of vitriol, contained in a phial with a ground ftopper and tube, (which is the method that I ufually employ to procure vitriolic acid air) and, with the flame of a candle, I made it boil.

The acid preſently became black, and the mixture yielded a great quantity of air, till it was quite viſcid; when, there being ſome danger of choaking the tube, I withdrew it. The air was at firſt half fixed air, making lime water turbid, and half inflammable, burning with a lambent blue flame; but towards the laſt two thirds of it was inflammable. I did not uſe more than a few pennyweights of the tartar, and the quantity of air exceeded two quarts, and much more might certainly have been procured. The next day the matter, which I had poured out of the phial, had the conſiſtency, colour, and ſmell of treacle; except that there were ſome ſmall concretions in it. Some time after I took the reſiduum above-mentioned, and putting it into a glaſs veſſel, I again extracted from it, in a ſand heat, a large quantity of air, as much as before, and exactly of the ſame kind. In the middle of the proceſs, when the production of air was moſt copious, it was very turbid; and when any of the bubbles burſt in the open air, they were perceived to have a ſtrong ſmell of treacle.

After

After this I ceaſed to make uſe of oil of vitriol, in order to try what air the tartar would yield of itſelf; and I preſently found that the acid had contributed nothing at all to the air that I had got from it. From an ounce of cream of tartar, in a glaſs veſſel, and a ſand heat, I got 170 ounce meaſures of air, the firſt portions of which were almoſt pure fixed air. The reſiduum, however, was inflammable, and burned with a blue flame. At laſt only about two thirds of the air was fixed air, and the reſt inflammable. In the greateſt part of the proceſs, the air was very turbid; but it was ſo in the recipient, and the part of the tube next to it, a conſiderable time before it was turbid in the reſt of the tube, or in the glaſs veſſel that contained the materials. Towards the end of the proceſs the empyreumatic oil came over, which was very offenſive, though, at firſt, the ſmell of the air had been rather pleaſant, reſembling that of burnt ſugar.

I repeated this experiment, and again got about 170 ounce meaſures of air from an ounce of cream of tartar, of which 38 ounce meaſures were inflammable, and the reſt fixed. It burned with a large white flame, but at laſt with a light blue one, owing, I ſuppoſe, to the mixture of fixed air in it.

That cream of tartar ſhould yield *fixed air* will not be thought extraordinary; but its yielding inflammable air, ſeems to ſhew that it had acquired a good deal of the conſiſtence of vegetable matter, or of pit-coal; ſince thoſe ſubſtances yield the ſame kind of air.

After this, neglecting the produce of air, I ſimply calcined a quantity of cream of tartar, in a red heat, in a glaſs veſſel filled up with ſand; and obſerved that it loſt about half its weight. Notwithſtanding its calcination in a red heat, this ſubſtance obſtinately retained a great deal of its fixed air, in which it reſembles chalk. For when I put this calcined cream of tartar into ſpirit of ſalt it yielded a conſiderable quantity of air, which I found to be fixed air, with a phlogiſticated reſiduum. It alſo, effervefced in the ſame manner, and no doubt gave the ſame kind of air in oil of of vitriol, and ſpirit of nitre. But even ſpirit of ſalt did not diſſolve the whole of it.

To obſerve the phenomena of this calcination more particularly, I made the proceſs in an open crucible, which I kept in a red heat a long time. But when there was no appearance of any farther change, and the ſubſtance was pretty hard, I took it from the fire, on which it preſently aſſumed a blackiſh, or dirty brown colour. Spirit of ſalt diſſolved this ſubſtance with as much rapidity, to all appearance, as it had done the mere black coal of tartar in the former experiment, and expelled as much air from it. It ſtill, however, did not diſſolve the whole: for a dirty powder remained undiſſolved.

Whether any chemiſt will think theſe obſervations of any value I cannot tell. Probably they are not of much conſequence, but I thought it might be worth while juſt to mention them.

SECTION XXXVII.

Miſcellaneous Obſervations on Subſtances expoſed to a long continued Heat.

MY experiments on expoſing ſubſtances to a long continued heat were begun, principally, with a view to aſcertain the converſion of water into earth, of which we have many credible accounts, and of which that excellent chemiſt Mr. Woulfe entertains no doubt.

For this purpoſe I provided glaſs tubes, about an inch in diameter, and three feet long, and alſo others made like what the workmen call *proofs*, growing narrower to the top, ſome two inches wide at the bottom, and others leſs than an inch. Indeed, I uſed glaſs tubes of a great variety of forms and ſizes, and when I had put in the water, or other fluid, I cloſed them hermetically, and placed them in a ſand furnace pretty equally heated. But, in general, before I placed them there, I expoſed the end containing the fluid

fluid near a common fire, for a few hours; both to obſerve whether there would be any immediate change, and alſo to try what degree of heat the tube, thus charged, would bear.

The reſult of many of the experiments made in this manner have been recited, and were ſufficiently remarkable, and others, that do not deſerve to be paſſed over will be noted in the courſe of this ſection. But with reſpect to *water*, which was my firſt and principal object, all my experiments intirely failed; and yet I do not therefore infer that the experiments of others have not been faithfully related, particularly thoſe of Mr. Godfrey.

In order to avoid expence, I uſed a greater degree of heat than had been uſed before for this purpoſe; hoping, by this means, to gain my end in leſs time. Whereas I believe Mr. Woulfe's opinion is quite right, *viz.* that the heat ſhould be very moderate, and long continued. Mine was conſiderably above a boiling heat in the open air, generally ſuch as to keep the water boiling in this confined ſtate, my veſſels being ſtrong in proportion. I went upon the idea, that the change of conſiſtence in water was brought about by extending the bounds of the repulſion of its particles, and at the ſame time preventing their actually receding from each other, till the ſpheres of attraction within thoſe of repulſion ſhould reach them. The hypotheſis may ſtill be not much amiſs, though I did not properly act upon it.

Be this as it will, a trial of ſix months had no effect of the kind that I hoped for. It ſhould, however, be conſidered, that it was ten months before Mr. Godfrey perceived any change in the conſiſtence of his water, and fifteen months before its converſion into earth was completed.

The particular appearances that I obſerved would be too tedious to relate, and were not of much importance. I ſhall, therefore, only obſerve in general, that I was deceived at the beginning of the proceſs, by finding that the whole maſs of water, which was generally an ounce, would become

become exactly like milk, and ſometimes the whole tube would have got a complete white coating in the courſe of a day or two. This I then hoped was, in part, a change in the water itſelf, though I had no doubt but that, in part, it might be owing to the corroſion of the glaſs by the heated vapour. In the end it appeared to have been nothing at all elſe.

When the heat was a little more moderate, the firſt appearance was a white pellicle on the ſurface of the water, and ſome times in the middle of the water only, not extending to the ſides; which deceived me the more into an opinion that this earthy pellicle might come from the water itſelf. In time there was ſuch an accumulation of this matter, that it clouded the whole maſs of the water, and ſunk to the bottom, in the form of white flakes, or a powdery ſubſtance. When the tubes were opened, all the ſides were found corroded, the poliſh being entirely taken off where the heat had been greateſt, eſpecially near the ſurface of the water.

I was

I was farther deceived by finding, on opening ſome of the tubes occaſionally, that when I had drained all the moiſture I could out of them, it weighed conſiderably leſs than it had done when it was put in, notwithſtanding a good deal of white flaky matter was neceſſarily poured out, and weighed along with the water, as well as a good deal left behind; and, with a view to theſe occaſional trials, I had moiſtened ſome of the tubes, letting them ſtand a ſhort time to drain before I put in the deſtined quantity of water. But when the proceſs was over, it appeared that much more moiſture had been entangled in that flaky matter that was left in the tube, and which could not be drained from it, than I had made allowance for, and much more than the weight of white matter that came out of the tube along with the water.

The force of the vapour of water in thus corroding glaſs is, however, not a little remarkable. In time it would have worked its way through any thickneſs of it. And, indeed, I ſhould obſerve, that the ſame is the

the caſe with iron. For before I began theſe experiments, I had made a few random trials of what might be done with water *in a ſhort time* by a very great degree of heat, in a confined ſtate, by putting the water into gun-barrels, then getting them cloſed by welding, and after that putting one end of them into a hot fire. Sometimes the water would continue thus a whole day or more; but at length though the gun-barrels were the thickeſt that I could meet with, and one of them was the breech of a muſket-barrel, and I believe perfectly ſound, it wore its way through. None of the barrels were properly *burſt*, but all of them were much corroded, and made exceedingly thin in particular places; and when they were opened a great quantity of ruſt was found in the inſides of them *.

Beſides trying the effect of this proceſs on pure diſtilled water, I made trial of water

* I ſince recollect that I formerly had a copper æolipyle, not leſs than the thickneſs of a half crown, which, after being uſed a good deal, burſt, and was found to be as thin as paper.

water impregnated with all the different kinds of air with which I am acquainted; and in other tubes the air confined along with the water was of all the different kinds; but the appearances in the mall were nearly the ſame, excepting ſuch as have been, or will be particularly deſcribed. The common air, in all theſe tubes, in which the water had been kept ſo hot, did not appear to have been changed either for the better or the worſe. Sometimes when I ſoftened a part of a tube with a blow pipe, the incloſed air would preſs the glaſs a little outwards, and ſometimes the external air would preſs it a little inwards, but it was with no great force; and whenever I opened the tubes under water, and examined the air, it did not appear to have been altered in its quality, with reſpect to its diminution by nitrous air.

It is known, that, in general, a menſtruum will hold more of a *ſolvend* when it is hot, than when it is cold; but theſe experiments in a continued heat afford ſeveral remarkable examples of the contrary. The firſt

firſt thing I obſerved of this kind was with reſpect to *lime water*: for having confined a quantity of it in one of my largeſt tubes, I found that, in ſix days, and how much leſs time might have ſufficed I cannot tell, all the lime was depoſited. At leaſt there ſeemed to be enough at the bottom of the water from which it was ſeparated, to have ſaturated the whole of it.

Alſo *iron* diſſolved in water impregnated with fixed air was ſeemingly all precipitated, in conſequence of being expoſed in the ſame manner to the heat; and when it was cold, it was not re-diſſolved. For though this menſtruum will diſſolve iron, it will not diſſolve the calx of iron. Perhaps the heated water might take the phlogiſton of the iron into a ſtate of more intimate union with itſelf, as in the experiments with quickſilver; in conſequence of which the calx of the iron, being deſerted by its phlogiſton, muſt of courſe be precipitated.

I had been informed by Mr. Bewly, that lime water would diſcharge the colour of

Pruſſian

Pruſſian blue. A quantity of lime water, thus impregnated with the colouring matter in Pruſſian blue, I put into one of my glaſs tubes on the 11th of Auguſt, and on the 23d, from being quite colourleſs, it was become of a greeniſh colour, with many opake particles in it. On the 9th of September following it was quite tranſparent, with a large white ſediment, in which it reſembled the tubes that had only water in them. This ſediment, therefore, might perhaps come from the corroſion of the glaſs. On the 30th of September, the liquor was quite cloudy, had a conſiderable precipitate, and a thick whitiſh incruſtation covered all the ſurface of it. Laſtly, on the 19th of January 1778, it had ſomething of a milky appearance, but was nearly tranſparent, and had depoſited a quantity of flaky matter.

Having the ſolution of *mercury*, and alſo of *copper* in ſpirit of nitre at hand, proper tubes to ſpare, and room enough for them in my hot ſand, I placed about an ounce meaſure of each of them in the furnace on the

the 9th of September, and on the 30th of the same month I found the solution of mercury quite colourless as at first; but I suppose the greatest part of the mercury was precipitated in one beautiful compact yellow mass. The precipitate of the copper was also collected into one mass, quite blue, as the liquor itself continued to be; so that the whole of the copper had not been precipitated.

When I took these tubes from the sand heat for a few days, the greatest part of the precipitated mass was re-dissolved; but when they were replaced in the sand heat they appeared again as at first; and so they were found on the 19th of January 1778, when an end was put to the process.

On the subject of the nitrous acid I shall observe, that water saturated with nitre, which had been placed in the sand furnace on the 3d of September, in a long and slender glass tube was transparent on the 30th of the same month; but the tube itself, from the surface of the liquor to half an inch below it, and likewise in different places

places quite to the top of the tube, was covered with a white incruſtation, a little inclined to blue.

Cauſtic alkali impregnated with nitrous vapour had cracked the tube in which it had been confined, and eſcaped; but the tube was found covered with a white incruſtation, from two inches above the ſurface of the liquor quite to the bottom of the tube. The crack itſelf was very remarkable, conſiſting, in reality, of many different cracks, and thoſe diſpoſed very irregularly, quite round the glaſs, near the ſurface of the liquor. I have ſometimes ſeen glaſs cracked in the ſame manner by electrical exploſions.

The moſt remarkable thing that I have obſerved, with reſpect to metallic ſolutions, relates to a ſolution of *gold in aqua regia*, made by the impregnation of the marine acid with nitrous vapour, which I have obſerved to be a more powerful menſtruum for gold than the common aqua regia. A ſmall quantity of this ſolution I had put into a very thick glaſs tube about nine inches

inches long, and I placed it in the ſand furnace on the 11th of Auguſt, and on the 23d of the ſame month I found much of the gold precipitated, and adhering to the ſides of the glaſs in the form of ſlender cryſtals, very beautiful. On the 30th of September, I obſerved no difference in the cryſtals, but found ſome gold precipitated in irregular maſſes, of a darkiſh colour, quite diſtinct from the cryſtals; and thus it remained till the 19th of January following, when I diſcontinued the proceſs. Both the cryſtals and the gold ſtill continue not re-diſſolved.

I ſhall now juſt mention my obſervations on ſome other ſubſtances expoſed to the ſame heat, though they have nothing in them that will be thought of any conſequence; except that it may be proper to be known that the experiments have been made, and that no remarkable appearance followed.

Spirit of wine in large tubes underwent no alteration, nor did it affect the glaſs in the leaſt; but another quantity confined in a ſhort tube, and expoſed to much more heat, appeared on the 30th of September (having been placed in the furnace on the 11th of the ſame month) to have given to the inſide of the tube, and eſpecially to the middle part of it, a thin blueiſh coating, a little inclined to white. Thus it continued to the laſt, except that the coating became more white, and had very nearly, if not wholly, loſt its blueiſh caſt.

Ether had alſo been confined in a ſhort and ſtrong tube on the 11th of Auguſt, and it continued colourleſs; but on the 30th of September ſeveral parts of the inſide of the tube had a whitiſh incruſtation, the glaſs being probably affected. Thus it continued till the end of the proceſs, in January following, except that I then obſerved the whitiſh incruſtation about an inch above the ſurface of the ether, at both ends of the tube, owing, I ſuppoſe, to my having, at different times, placed both the ends downwards.

With ether I alſo made another experiment ſomewhat ſimilar to the above. Having

Having filled a glaſs tube with it, I poured it out again, and immediately ſealed it hermetically; then holding it in the flame of a candle, I obſerved a whitiſh cloud formed in the inſide, and when the whole tube was expoſed to the heat of the fire, and was made nearly red hot, part of it became whitiſh; but the air within the tube was not ſenſibly changed. I made the experiment in imitation of that with the inflammable air, which made the tube become black; thinking that, if the phlogiſtic matter had produced that effect in this caſe, it might do the ſame in another.

Olive oil expoſed to a very great degree of heat, in a ſhort and ſtrong tube, was not changed. But in a large tube (owing, I imagine, to ſome bit of ſtraw, or ſome other ſubſtance containing phlogiſton, which, unperceived by me, might be in the tube) the oil became, in the interval between the 11th and the 23d of Auguſt, quite black, and of the conſiſtence of treacle, with a ſmell ſtrongly empyreumatic and offenſive. I put part of this matter into another tube, but it was broke by ſome accident, and what remained of the matter was as hard as a coal, and quite black.

Oil of Turpentine, which was quite colourleſs, became, in the ſame time, quite yellow, like dark coloured olive oil. It had alſo ſome opake particles in it. The glaſs being ſoftened, it was preſſed inwards. On the 9th of Sept. the colour of the general maſs was the ſame, but there were ſeveral ſmall lumps at the bottom, exactly like roſin to appearance. They did not adhere to the glaſs, but rolled about at the bottom, being heavier than the fluid maſs. In a ſhort glaſs tube, alſo, oil of turpentine was a little yellow.

Diſtilled Vinegar ſuffered no change by being expoſed in a long glaſs tube to a common fire for about an hour. But common vinegar, in the ſand furnace, was turned almoſt black in the courſe of three weeks. But I aſcribe this effect to ſome phlogiſtic matter contained in it. After the proceſs, the taſte of it was evidently leſs acid, like vapid vinegar, and the air within the tube

was

was injured ; one meaſure of this and one of nitrous air occupying the ſpace of 1.4 meaſures.

After this I placed diſtilled vinegar in the ſand furnace ; and this, in the interval between the 9th and the 30th of September, had made a depoſit of ſome black matter, and the tube was coated with it quite round, at the ſurface of the liquor. Alſo, in a ſhort tube, the ſame vinegar was a little opaque, and there was ſome black matter on one ſide of the tube, half an inch above the ſurface of the fluid. In this ſtate theſe tubes continued to the laſt, when they had depoſited a browniſh ſediment.

Having expoſed a ſmall quantity of water impregnated with *fluor acid air*, quite tranſparent, in a glaſs tube hermetically ſealed, to the heat of a common fire, I obſerved that, preſently after it began to boil, it became of a dull blue colour, and a whitiſh vapour roſe from it, as high as the middle of the tube. Afterwards, the heat increaſing, it became tranſparent again, without depoſiting any thing, even when cold.

Repeating the ſame proceſs, I obſerved the ſame cloudineſs come on after boiling about an hour, but after continuing to boil two or three hours, it diſappeared again. This cloudineſs is exactly like the appearance of this impregnated water when ſome of the fluor cruſt is mixed with it. This experiment, therefore, proves that this liquor, in its moſt tranſparent ſtate, contains a quantity of fluor cruſt diſſolved in it, as I have obſerved before, in my attempts to account for its not freezing, when water impregnated with vitriolic acid air will freeze.

The effect of a continued heat on the *volatile alkaline liquor* was much the ſame with that on the acid impregnations. I expoſed, in a glaſs tube, four feet long, and one third of an inch wide, a quantity filling about the ſpace of an inch of cauſtic ſal-ammoniac bought at the apothecaries; and in leſs than half an hour it became turbid, when over the fire. Letting it cool, I ſoftened the end of the tube, and obſerved that the glaſs was preſſed inwards. I then made it boil very violently about an hour, during which it

grew

grew more turbid. When it was cool, I obſerved that the turbidneſs was occaſioned by very ſmall white particles, which ſubſided, and left the liquor quite clear at the top. Softening the end of the tube again, it was driven outwards with great force, and blew out the candle; ſo that, upon the whole, there had been an increaſe of elaſtic matter within the tube, notwithſtanding the precipitation.

After this, I placed in the ſand furnace an alkaline liquor of my own preparing, by impregnating diſtilled water with alkaline air. It was confined in a long tube, a quarter of an inch in diameter, on the 3d of Sept. and on the 9th of the ſame month the tube was quite coated with a white ſubſtance, and the liquor was turbid. On the 30th of the ſame month it had depoſited a white ſediment, though it was ſtill very turbid. There was alſo a ſimilar incruſtation at the ſurface of the liquor, and extending in ſtreaks three inches above it. At the ſame time, that which had been bought at the apothecary's, and which had been placed in the ſame furnace exhibited the ſame appearance. In this the incruſtation reached ſix inches above the ſurface of the liquor, eſpecially on the ſide to which it had been inclined. One of theſe tubes remained in the hot ſand till the 19th of January following, when I found it broken; five or ſix inches of the lower part of it being covered with a thick white incruſtation.

Common air, which had been confined in a glaſs tube hermetically ſealed, and quite covered with hot ſand about a week, was not at all altered in its bulk, or with reſpect to its property of being diminiſhed by nitrous air.

Several of the obſervations in this volume will, I hope, be acceptable to Mr. Delaval, and may perhaps be of ſome uſe to him in a future edition of his excellent *treatiſe on colours*. Among others, the following ſeems to agree with ſome of the wonderfully regular gradations obſerved by him. A ſmall quantity of the blue ſolution of copper in ſal ammoniac, being expoſed to the heat of a common fire, in a long glafs tube hermetically fealed, prefently became green, and afterwards yellow.

SECTION XXXVIII.

Experiments in Electricity.

THAT *conducting power*, with refpect to electricity, depends upon the variable ftate of fubftances, is evident from a variety of experiments. Thus glafs, which when cold is a perfect nonconductor, is a complete conductor in a great degree of heat. So alfo, by a contrary procefs, *ice*, which when formed in a moderate degree of cold is a conductor, very much like water, becomes, as Mr. Achard has difcovered, a nonconductor in a greater degree of cold. And I had found that though *dry wood*, and even *charcoal*, made with the leaft poffible degree of heat, is a nonconductor. yet when it has been expofed to *more heat*, it is the moft perfect of all conductors, not exceeded even by the moft perfect metals themfelves.

themſelves. I have now obſerved what, indeed, was not perhaps very difficult to be conjectured, that *water*, and even *quickſilver* in the ſtate of vapour, are no conductors of electricity.

Water had been often tried in that kind of vapour which is juſt condenſing, in the open air; but then it is, in fact, no other than water in very ſmall drops; whereas, to try it in the proper form of *ſteam*, it muſt be examined in a degree of heat, in which it is incapable of condenſing into water. This I did in the following manner:

I filled a glaſs ſyphon with water, having previouſly put iron wires into each of its legs, as is repreſented Fig. 5. and then inverted it, placing each leg in a ſeparate baſon of water, or quickſilver. After this I expoſed the upper part of the ſyphon to a degree of heat capable of converting water into ſteam. Then, bringing a charged phial, and making the ſyphon part of the circuit, made the exploſion paſs from one wire to the other, in the bend of the ſyphon. In this caſe the ſpark never failed to be as viſible, as it would

have

have been in the air. The only difference was, that in this caſe the ſpark was reddiſh, as it is when taken in inflammable air. I could perceive no difference whether the heat was greater or leſs, even in the very point of condenſing into water. It is poſſible, however, that there might be ſome real difference, though not diſcernible in this method of examining it.

In the very ſame manner, I made the experiment in the vapour of *quickſilver*, having filled the ſyphon with quickſilver, and placing the legs of it in baſons of the ſame. In this caſe, alſo, the electric exploſion was red; but at one time it was quite vivid. I repeated the experiments many times, both with water and with quickſilver.

From theſe experiments, compared with ſimilar ones that I have made in all the different kinds of air, I think it may be concluded univerſally, that all ſubſtances, in this expanded ſtate of air, or vapour, are nonconductors of electricity.

There is ſomething exceedingly difficult to account for in the circumſtances in which

glaſs

glaſs jars ſometimes break ſpontaneouſly with electrical exploſions. In general the thinner the glaſs is, the more liable it is to a fracture in this caſe. I obſerved, however, in my *hiſtory of electricity*, a caſe in which a very thick glaſs jar broke, in a very remarkable manner, by a ſpontaneous diſcharge; and I have lately obſerved another hardly leſs remarkable.

I filled a glaſs tube, about three feet long and $1\frac{1}{4}$ of an inch wide, the glaſs itſelf being not leſs than one eighth of an inch thick, half full of quickſilver; and putting a looſe coating of tinfoil on the outſide, and beginning to charge it, by means of an iron wire connected with the prime conductor, it preſently broke by a ſpontaneous diſcharge, exactly at the bottom. A large piece of the glaſs came out, and the quickſilver flowed out at the hole. Examining it more particularly, it appeared that there were a great number of ſmall independent fractures, but all very near together; and through one of them only the charge had made its way, pulverizing the glaſs, as uſual.

I then

I then charged a long tube of *bottle glaſs* in the ſame manner; but this alſo burſt as ſoon, and alſo exactly at the bottom, though not in ſo many places. I meant to have charged theſe tubes, and to have ſealed them hermetically, after I had poured out the quickſilver, in order to obſerve how long ſo thick a glaſs would retain the charge, in purſuance of Mr. Canton's firſt obſervation of this kind.

SECTION XXXIX.

MISCELLANEOUS EXPERIMENTS.

1. *Of the Colour of Minium.*

AS I was heating a quantity of minium in an iron ladle, I was very much ſtruck with the reſemblance of its colour, and of the change of its colour, to that of blood. The colour of good minium is, as nearly as poſſible, that of florid, or what I call, dephlogiſticated blood. It is the colour they both acquire from expoſure to the air. When the minium was in the ladle over the fire, the ſurface continued of this colour, but

but all the lower part of the mafs was of a deep red, or black, the colour of dark coloured, or phlogifticated blood. But, like blood (only, in this cafe, the procefs was much quicker) the moment that any part of it was turned up to the open air, it refumed its florid light colour; and when it was cold, it could not have been perceived that any thing had been done to it. However, when I expofed a quantity of minium that had been treated in this manner to a red heat, in a glafs veffel, though it yielded about the fame quantity of dephlogifticated air that I imagine it would have done before, it yielded much lefs fixed air.

Imagining that this dark colour might be the confequence of the minium receiving phlogifton from the iron, I expofed a quantity of it to the fame degree of heat in a glafs tube, but found the fame change of colour. In this, therefore, it refembles the change of colour in fpirit of nitre, which is produced by heat only, without the help of any additional phlogifton, unlefs any may be fuppofed to pafs through the glafs.

The

The tube was ſeveral feet long, and was quite filled with the minium; and preſently after it was expoſed to the heat of the fire, the colour began to change, growing darker and darker continually, till it was almoſt black, exactly as it had done in the iron ladle. But when it was cold, it re-aſſumed its florid light colour. That it ſhould do this without the acceſs of the external air rather ſurprized me; and yet that no air, except what was contained in the interſtices of the minium itſelf, had acceſs to it was evident from the lower part of the glaſs being ready to burſt with the expanſion of the air, when it was in a melting heat.

It was obſervable, that from the black colour, the minium paſſed, without any ſenſible interval, into yellow, in which ſtate it contains little or no air of any kind; ſo that the florid colour is an indication of its containing pure air, whatever be the connection between theſe circumſtances. It muſt be obſerved, however, that minium deprived of its red colour by ſpirit of ſalt does

does not loſe its property of yielding de-phlogiſticated air.

2. *Of the Mixture of Vitriolic Acid Air, and Fluor Acid Air.*

In a former publication, I obſerved, that when once any two kinds of air are mixed together, they do not, at leaſt, they do not *ſoon*, or readily, ſeparate from each other, though their ſpecific gravities be ever ſo different, but continue equally mixed through the whole maſs. I then made the experiment on thoſe kinds of air that can bear to be confined by water. I would now obſerve, that the ſame is the caſe with common air and alkaline, or any of the acid airs. For though all theſe kinds of air differ in ſpecific gravity from common air, yet if they be mixed with common air, and water be admitted to them, the quantity will decreaſe more or leſs ſlowly in proportion to the quantity of common air in the mixture. Whereas, if the alkaline or acid airs had been heavier than the common air (as the latter, at leaſt, manifeſtly

manifeſtly are) and did not mix with it, the water would abſorb them as readily as it does when the jar contains no other kind of air; as, on the other hand, if the common air had been the heavier, it would have protected them from the acceſs of the water, which would not, in this caſe, be able to come at the acid or alkaline air, and therefore could not abſorb any part of the quantity. I have noted, however, one exception to this rule reſpecting alkaline and inflammable air, which did not ſeem to mix together. See Vol. I. p. 176.

I have ſince made a mixture of vitriolic acid air and fluor acid air, and find that they continue intermixed throughout. I mixed equal quantities of them in a jar of quickſilver, and obſerved, that when water was admitted to the whole maſs, the cruſt was formed equably from the bottom to the top of the veſſel.

3. *Of Fluor Acid Air corroding Glaſs.*

Fluor acid air, when it is firſt produced, corrodes the glaſs veſſel in which it is

 generated.

generated. But whether it did this of itſelf, merely in conſequence of being heated, or whether the moiſture, or ſomething elſe contained in the oil of vitriol, by means of which it is formed, contributed to this effect, did not certainly appear. When this air is cold, it does not at all affect the glaſs veſſel in which it is confined. In my late attempts to confine the different kinds of air in glaſs tubes hermetically ſealed, in order to expoſe them to a continued heat, I obſerved that it is ſimply the *heated air* that has this effect. For when I had filled a tube with this kind of air, and was endeavouring to take off different lengths of it, with a blow pipe, I found that, when the glaſs became red hot, it was always ſo corroded, and diſſolved, that it was impoſſible to cloſe it by ſealing.

4. *Common Air affected by heated Quickſilver.*

In a former publication I brought ſome arguments to ſhow that there is no air in quickſilver; as has generally been imagined, and that all the air which is diſcovered in boiling

boiling it in a glaſs tube, is only that which had been concealed, and compreſſed, between the quickſilver and the glaſs. Having then collected a ſmall quantity of this air, I obſerved that I found it to be common air, being diminiſhed by nitrous air. But the quantity being ſmall, and not having applied a very accurate meaſure, I have ſince repeated that experiment with more precaution, and find ſuch air to be in ſome degree phlogiſticated; but this, I imagine, ariſes, from the phlogiſton eſcaping from the quickſilver, eſpecially when it is hot.

I firſt filled a tall thin tube, about an inch in diameter, with quickſilver; and, expoſing the upper part of it to a degree of heat that converted it into vapour, in the manner repreſented Fig. 4. and conſequently effectually ſetting at liberty all the air that was confined between the quickſilver and the glaſs, I collected and examined that air, and found it not to be diminiſhed by nitrous ſo much as common air is.

 I then

I then repeated the experiment by throw-up a quantity of common air, and expoſing it to heat mixed with the vapour of quick-ſilver, and let it continue in that ſtate four or five hours. After this I perceived that the air was conſiderably diminiſhed in bulk; and, examining it, I found that one meaſure of it and one of nitrous air occupied the ſpace of 1.66 meaſures. The air, therefore, in the former experiment, not having been pure air, is no proof of its having been incorporated with the quick-ſilver; ſince common air mixed with it, in the ſtate of vapour, receives phlogiſton from it. This proves that, like other metals, quickſilver is diſpoſed to part with phlogiſton to the air when it is hot. Query, what becomes of the calx of mercury to which the diſcharged phlogiſton belonged?

4. *Of the Mixture of the Vitriolic and the Nitrous Acids.*

Becauſe a mixture of nitrous acid will diſcharge the black colour from phlogiſticated vitriolic acid, Mr. Beaumé infers that

that the former has a ſtronger affinity with phlogiſton than the latter. He alſo obſerves of this mixture that it will readily inflame oil of turpentine, but that nothing farther is known concerning it.

I would obſerve, however, that the vitriolic acid does likewiſe diſcharge all colour from the nitrous acid, and therefore, reaſoning as Mr. Beaumé does, we might draw a concluſion the reverſe of his. I would therefore rather ſay, that the two acids in conjunction have a different action upon phlogiſton than they have when ſeparate.

If the marine acid be mixed with the vitriolic, the marine acid air is inſtantly expelled and the water is, I ſuppoſe, ſeized by the acid of vitriol. But when the vitriolic and nitrous acids are mixed, no ſuch effect takes place. They, therefore, ſeem to occupy the water jointly, without either of them diſlodging the other, at leaſt in the ſpace of ſome weeks. What more time will effect I have not yet ſeen.

If the nitrous acid be poured gently upon the vitriolic, ſtrongly concentrated, they will continue unmixed for ſome time; but, without any agitation, they will incorporate gradually, a white cloudineſs being always ſeen where they are contiguous. When they are ſhaken together a ſmall degree of heat will be produced, and numberleſs bubbles will be formed, which, however, are preſently abſorbed. There is alſo at firſt a whitiſh vapour over the ſurface of the mixture; and after ſome time, though both the acids be ever ſo pure, and the vitriolic has been diſtilled again and again, there will be a depoſit of a white ſubſtance, which I have not yet examined.

I have obſerved that the yellow colour of the common ſpirit of nitre is diſcharged by a mixture of the vitriolic acid. When I poured a weak green ſpirit of nitre upon concentrated oil of vitriol, it became yellow where they were contiguous; but the quantity of nitrous acid being much greater than that of the vitriolic, it was green above, without any viſible vapour on its ſurface. The next morning the nitrous acid was

colourleſs,

colourleſs, contiguous to the vitriolic, and the reſt yellow.

Afterwards I poured upon concentrated oil of vitriol an equal quantity of that nitrous acid, which had firſt acquired a deep orange colour by heat, and then had become green by keeping. The effect was, that from green it inſtantly became yellow throughout, and continued diſtinct from the vitriolic acid ſix days. In one day they did not ſeem to affect each other in the leaſt, but afterwards a cloudineſs was obſerved, where they were contiguous to each other, which increaſed till almoſt the whole had that appearance; and when they were ſhaken together it was tranſparent like water.

In order to try the full power of the vitriolic acid to diſcharge the colour of ſpirit of nitre, I diſſolved in the ſtrongeſt ſpirit of nitre a quantity of copper, which gave it a deep green colour. But on mixing it with vitriolic acid it inſtantly became perfectly colourleſs, and the copper was precipitated in the form of a white powder.

I poured very gently a quantity of aqua regia, made by impregnating marine acid with nitrous vapour, on vitriolic acid, and at firſt it effervefced very much, and the lower part was of a turbid white, while the upper part retained its orange colour. After ſome time the mixture was of a light orange throughout. I have not yet made any farther obſervations upon it.

To try how ſtrongly the nitrous acid vapour was retained in this mixture of the two acids, I expoſed a part of the mixture to the heat of a common fire, in a long green glaſs tube hermetically ſealed, and found that though I kept it boiling, it continued colourleſs a conſiderable time. Afterwards a red vapour was expelled from the mixture, and at length the whole tube was filled with it. But when it was cold the vapour was all abſorbed again, and the mixture, which was then of a pale orange colour, became afterwards quite colourleſs, as at firſt. This is not the caſe with oil of vitriol impregnated with nitrous vapour. For this vapour eſcapes

escapes from it even without heat, and much more with it, and it is not re-absorbed.

5. *Of a Solution of Copper in strong nitrous Acid.*

It is something remarkable that though a great quantity of nitrous air is produced by the solution of copper in a diluted nitrous acid, no air at all is procured by a solution of the same metal in the strong acid. There is not even any appearance of air being formed, and afterwards absorbed by the acid, as in the similar solution of mercury.

Having saturated a quantity of strong spirit of nitre with copper, of which it dissolves but a small quantity, I distilled it in a green glass retort. The first part of the acid that came over was orange coloured, from being of a deep green; but the last was quite transparent and weak. No air, that I could perceive, was produced, but a tubulated receiver being made use of, a small quantity could not be discovered.

6. *Of*

6. *Of Air from Minium, diſſolved in Spirit of Salt.*

Spirit of ſalt, I have obſerved, diſſolves a great quantity of minium. In order to diſcover what became of the dephlogiſticated air it contains, I diſtilled a quantity of that ſolution, which was of a yellow colour, made by the firſt affuſion of the acid. When the ſolution became hot it yielded a quantity of dephlogiſticated air, mixed with a very ſmall quantity of fixed air, ſo as to make lime water turbid only in the ſlighteſt degree. As it boiled no air at all was procured, nor when it was diſtilled to dryneſs.

I treated in the ſame manner a ſaturated ſolution of white minium, made ſo by its colour having been diſcharged by a previous affuſion of the acid. But this ſolution yielded no air at all from the beginning to the end of the proceſs. Nor was the common air in the retort phlogiſticated either at the beginning or the end.

7. *Experiments with Froſt.*

I have obſerved, Vol. III. p. 360, that water impregnated with vitriolic acid air eaſily freezes, retaining all its air, which is a pretty extraordinary fact; being the reverſe, in one reſpect, of water impregnated with marine acid air, which cannot freeze; and, in another reſpect, of water impregnated with fixed air, which in freezing parts with its air. At the ſame time I obſerved that water impregnated with fluor acid air did not freeze. I now find that the latter fluid does freeze, though it requires a greater degree of cold than water impregnated with vitriolic acid air. The latter effect I attributed to the preſence of ſome of the fluor cruſt in the ſolution, and I think this conjecture is, in ſome meaſure, confirmed by the following obſervations; in which it will be ſeen, that lime water did not freeze ſo ſoon as common water, and that lime water impregnated with vitriolic acid air did not freeze ſo ſoon as common water ſo impregnated.

Jan. 7, 1779. I expoſed to the cold all night a phial of pump water, and one of the ſame water ſaturated with quick lime. The next morning I found the thermometer at 28, the pump water frozen ſolid, but the lime water not frozen at all.

Jan. 9. When the thermometer was at leaſt 23 water impregnated with fluor acid air, after being expoſed to the cold all night, was imperfectly frozen. At the ſame time water impregnated with vitriolic acid air was quite ſolid, and alſo a quantity of the ſame in which ſome chalk had been diſſolved. But lime water impregnated with vitriolic acid air was quite fluid. Lime water was frozen, and a little of the lime was precipitated.

Jan. 12. When the thermometer was at 20, and had probably been lower in the night, I found the lime water impregnated with vitriolic acid air, and alſo the water impregnated with fluor acid air, ſolid throughout. The former was quite white, but was tranſparent again when the ice melted. As the ice of the fluor acid melted, it ſwam on the ſurface of the liquid part.

8. Of

8. *Of a Saline Subſtance formed by Earth of Alum and fixed Air.*

At the time that I firſt heard of Mr. Achard's capital diſcovery of the formation of cryſtals from various earthy ſubſtances and fixed air, I endeavoured to ſimplify his proceſs (which requires a good deal of attention, as well as an expenſive apparatus, and of difficult conſtruction) and among other things I fully ſaturated with earth of alum a quantity of water, impregnated with fixed air, and I let an ounce phial of it, with a redundancy of earth of alum in it, remain ſome months, in which time a great part of the water was evaporated. But after that time I found in the ſediment *a ſaline ſubſtance*, conſiſting of two cones, on the ſame baſe, each having ſix ſides, and the whole weighing five or ſix grains. It had a peculiar taſte, ſomething like that of alum. Having had it in my mouth ſeveral times before I thought of weighing it, I cannot be quite certain what its original weight was. I had flattered myſelf with the expectation of a difa different kind of ſubſtance from this proceſs.

9. *Remarks on the Article* GAS *in the new Edition of Mr.* MACQUER'*s Dictionary of Chemiſtry.*

That excellent chemiſt, and moſt perſpicuous of writers, Mr. Macquer, has, in a new edition of his valuable *Dictionary*, given a large article on the ſubject of the different kinds of air, under the article *gas*, which I think very judicious, and uſeful in moſt reſpects, as well as highly flattering to myſelf. But as he ſeems to me to have made a few miſtakes, I think I ſhall oblige him, and others, by endeavouring briefly to point them out.

He agrees with Mr. Lavoiſier in ſuppoſing that phlogiſton, combined with common air, converts it into fixed air, p. 260, 292, &c. and he imagines, that I ſuppoſe air to be injured by a mixture of fixed air, and that plants reſtore noxious air by imbibing that fixed air, p. 293. Agreeably to this idea, which runs through the whole article

article, he fays, that the agitation of fixed air in water makes it approach to the nature of wholefome air, p. 254, and that a mixture of nitrous air with common air converts it into fixed air. He even exprefsly fays, p. 297, that the union of phlogifton with air diminifhes its quantity, increafes its fpecific gravity, renders it unfit for refpiration or combuftion, and makes it approach to the nature of fixed air, by paffing through the ftate of phlogifticated air.

On this fubject, however, this ingenious writer does not give *my* opinion, or one that is agreeable to fact. For air fimply injured by phlogifton is not heavier, but lighter than common air; and not making lime water turbid, or being peculiarly liable to be abforbed by water, it fhews no fign of approaching to the nature of fixed air, which is, moreover, heavier than common air; nor will any length of time, or addition of more phlogifton, tend, in the leaft, that I know, to bring it to this ftate.

On the contrary, it will rather follow from my obfervations, that fixed air is con-

vertible

vertible into phlogiſticated air, and this into pure air, by more proceſſes than one, and eſpecially by incorporating with water, by which a portion of any quantity of fixed air is converted into phlogiſticated air. Conſequently, by repeating the proceſs, the whole would become ſo; and phlogiſticated air is by various proceſſes convertible into pure air. So that fixed air may rather be called the medium between pure air and phlogiſticated air, and not phlogiſticated air the medium between pure air and fixed air.

He aſſerts, with Mr. Lavoiſier, p. 298, that metalic calces with the addition of combuſtible ſubſtances, yield fixed air, a miſtake on which I have animadverted already.

He mentions, p. 377, the *vegetable acid air* as my diſcovery. But though I have a ſection on that ſubject, Vol. II. p. 23, I obſerved in the ſame volume, p. 334, that, not having been able to get any air from *radical vinegar*, and finding that vitriolic acid had been employed in making the concentrated vinegar from which I had extracted

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that air, the properties of which I had deſcribed under the title above-mentioned, I concluded that it was, in fact, the vitriolic acid air, though perhaps a little modified; and that, properly ſpeaking, there is no ſuch thing as a vegetable acid air.

He ſays, p. 313, that I ſpeak of fixed air as not leſſening the inflammability of inflammable air, the contrary of which he had himſelf obſerved. What I have ſaid is, that when fixed air and inflammable air have been mixed together, water will abſorb the fixed air, and leave the inflammable air poſſeſſed of its original properties. Inflammable air itſelf, I obſerve, will extinguiſh a red hot coal, and that it cannot be ignited with a candle, but by the help of common air, as in its iſſuing out of the mouth of a phial.

SECTION XL.

Experiments and Obſervations made ſince the preceding Sections were ſent to the Preſs.

§ 1. *Of Oil of Vitriol impregnated with nitrous Vapour.*

I HAVE deſcribed a number of beautiful *feather-like cryſtals* formed in ſome phials containing oil of vitriol impregnated with nitrous vapour. Cryſtals ſimilar to theſe may be produced at pleaſure, if the vitriolic acid be highly concentrated, and the nitrous vapour very copious; but they will appear on the ſides of the phial, and not in the body of the acid itſelf.

When the vitriolic acid is nearly ſaturated with the nitrous vapour, hold the phial (which ſhould be a large one, containing about a quart) and turn it ſo as to moiſten all the inſide of it. Then immediately throw in a very copious nitrous vapour, ſo that the whole phial ſhall be intenſely red, and running over; after which put in the ſtopper, and

let

let it remain quite ſtill. The upper part of the oil of vitriol will then be of an orange colour, and all the ſides of the phial, and eſpecially the parts towards the bottom, will ſoon be quite covered with thoſe cryſtals, but of different ſizes. By degrees they will be formed on the ſurface of the acid; but in a few hours afterwards, when the nitrous vapour is equally diſtributed through the body of the oil of vitriol, all theſe cryſtals will diſappear.

By repeating this proceſs, one half of the whole body of vitriolic acid will be cryſtallized in an irregular manner, as if it was congealed. When I have poured the whole of this ſemi-congealed maſs into a ſmaller phial, juſt large enough to contain it, the coagulated part has ſubſided to the bottom, and other cryſtals have gradually formed, ſhooting with ſome regularity from it into the middle of the ſuperincumbent liquid, which has always become more pellucid, and approached more to the colour of ſpirit of nitre, in proportion as the cryſtals have extended themſelves.

Finding that all the acid of vitriol was contained in the cryſtals, and that the ſuper-incumbent liquid became in time pure ſpirit of nitre, I was deſirous of knowing whether, if there ſhould be any phlogiſtic matter previouſly contained in the oil of vitriol, the phlogiſton would be retained in the cryſtals, or paſs into the ſpirit of nitre.

With this view I diſſolved a ſmall quantity of bees-wax in highly concentrated oil of vitriol, making it thoroughly black, and greatly increaſing its viſcidity; and afterwards I impregnated it with nitrous vapour, and ſhut it cloſe up in a ſmall phial. After ſome weeks the cryſtals began to form, and they were intirely white, juſt as if the vitriolic acid had been pure. The proceſs is not yet completed; but I expect that the nitrous acid will be highly phlogiſticated. Does not this experiment ſeem to prove, that the nitrous acid has a ſtronger affinity with phlogiſton than the vitriolic? The fact is certainly a pretty remarkable one.

§ 2.

§ 2. *Of the Colour of the nitrous Acid.*

I have obſerved that *heat* never fails to give a high orange colour to the paleſt ſpirit of nitre, and that with the leſs heat the acid is made, the lighter the colour of it will be. Having purpoſely made the proceſs for diſtilling this acid with as little heat as poſſible, and taking care to have no phlogiſtic matter in the materials, I procured a large quantity of the acid (that which came in the middle of the diſtillation) as nearly as poſſible quite colourleſs, like water, and yet of the ſtrongeſt ſort.

I have alſo obſerved a farther, and a very remarkable change of colour in the phlogiſticated nitrous acid, after being kept a long time in phials with good glaſs ſtoppers. For from being of the deepeſt orange, it has become quite *green*, the ſuperincumbent vapour continuing ſtill of an orange colour.

This change I firſt obſerved in a conſiderable quantity of nitrous acid which had been of a light ſtraw colour, and had aſſumed the deepeſt orange, by expoſure to heat in a glaſs tube hermetically ſealed. This was alſo the caſe with ſeveral quantities of the acid incumbent on the cryſtals of oil of vitriol, of which I have made frequent mention; and in one of the phials it had paſſed from green to a *deep blue*.

I muſt alſo take notice, in illuſtration of this fact, that, in the proceſs for producing the nitrous vapour, viz. the rapid ſolution of biſmuth, the liquid that comes over, mixed with the vapour, and which drops now and then from the end of the tube out of which the vapour iſſues, is generally of a deep blue.

Laſtly, if a quantity of this deep green acid be put into a large phial, where the vapour has liberty to expand itſelf, it reſumes its orange colour. This I have alſo obſerved is the caſe on pouring it on concentrated vitriolic acid.

§ 3. *Of nitrous Air imbibed by Charcoal.*

I dropped a piece of red hot charcoal into a phial of nitrous air, and immediately inverting it in a baſon of mercury, the air was

was preſently reduced to one fifth of the whole. Thus it continued two months, without any ſenſible change; after which I found that the air that remained unabſorbed did not affect common air, nor did the air that was emitted by the charcoal, when it was plunged in water; ſo that, in both theſe caſes, the air ſeems to be intirely deprived of its peculiar properties, and to become mere phlogiſticated air.

§ 4. *Of nitrous air being, to Appearance, converted into Inflammable Air.*

I have mentioned a caſe, Vol. I. p. 217, in which nitrous air, after having been expoſed to iron, became not only partially inflammable, admitting a candle to burn in it with an enlarged flame, but was even fired with an exploſion, like inflammable air from metals by oil of vitriol. I have ſince met with a more remarkable fact of the kind.

At the latter end of September 1778, I had put a pot of iron filings and brimſtone into a jar of nitrous air, which, in the courſe of ſeveral days, was diminiſhed by it in the uſual proportion. From that time till the beginning of December it had continued without any change that I had perceived; but about that time, imagining it was increaſed in bulk, I took exact notice of the dimenſions of it, and preſently found that the quantity was certainly increaſing. Upon the whole, I concluded that it had increaſed about one ſixth of its bulk, from the ſtate of its greateſt diminution. On the 11th of December I examined it, and found it to be proper inflammable air, being fired with many exploſions when tried in the uſual manner, but they were not ſo vigorous as thoſe with freſh made inflammable air from iron and oil of vitriol.

After this, on the 12th of December, I put a pot of iron filings and brimſtone to another quantity of nitrous air, and on the 4th of February following it had increaſed in bulk about one third, and then burned with exploſions like the former. But a quantity of nitrous air expoſed to the effluvium of liver of ſulphur, the very ſame time, never

never increaſed at all after the period of its utmoſt diminution, and was mere phlogiſticated air.

The circumſtance that makes it rather probable that here was a converſion of nitrous air into inflammable, is that I have never found air of any kind to come from this mixture of iron filings and brimſtone, except in a conſiderable degree of heat; and to give it what I thought a fair trial, I confined it at one time under water, Vol. I. p. 108. But I never kept it in thoſe circumſtances more than a week or a fortnight. Perhaps more *time* may produce the ſame effect as *heat*, and thus a quantity of inflammable air may be added to the phlogiſticated reſiduum of the decompoſed nitrous air. But then the exploſions ſeemed to be rather too vigorous for that proportion of inflammable air in the phlogiſticated air.

To try whether, after the uſual diminution of common air by this proceſs, there would, in length of time, be a generation of inflammable air, I put a large pot of iron filings and brimſtone into a ſmall quantity of common air, and on the 4th of February following, when I was obliged to put an end to the experiment (but it was the ſame time in which the nitrous air had become inflammable) though there was an increaſe of about one twelfth from the ſtate of its greateſt diminution, there was nothing ſenſibly inflammable in it. It was mere phlogiſticated air. What will be the effect of more time with this proceſs I cannot tell, and therefore I do by no means determine whether the nitrous air was changed into inflammable air; or whether, being firſt decompoſed, and become phlogiſticated air, there was an addition of inflammable air made to it.

§ 5. *Of the different Effects of Liver of Sulphur, and Flowers of Zinc on coloured Spirit of Salt*

Both liver of ſulphur and flowers of zinc, I have obſerved, diſcharge the colour of ſpirit of ſalt. But when I diſcharged the colour of a quantity of this acid, made

2 very

very yellow with various impregnations, with liver of ſulphur, it recovered its colour by being expoſed to the open air. On the contrary, though flowers of zinc produced the ſame effect, in diſcharging the colour of another portion of the ſame acid, the colour did not return by expoſure to the air, not even though liver of ſulphur was afterwards put to it.

§ 6. *Of the Effect of Marine Acid Air on Flowers of Zinc, &c.*

Being deſirous of aſcertaining whether the marine acid air would combine with the ſame ſubſtances that the marine acid diſſolved, I made the trial with the flowers of zinc and red lead; and found that both theſe ſubſtances abſorbed a very great quantity of that air. I therefore conclude that whether the marine acid be combined with water, or not, it has the ſame affinity with theſe earthy ſubſtances.

THE

APPENDIX.

NUMBER I.

A Letter from Sir William Lee, *Baronet, to Dr.* Priestley, *on the Use of Water impregnated with fixed Air, in preserving Flesh Meat from Putrefaction.*

Sir,

YOUR benevolence will, I am sure, excuse the present trouble I give you, however intrusive and otherwise unwarranted, as it proceeds from a like disposition, and is occasioned by your own experiments so judiciously directed to the service of the public.

The uncommon difficulty of keeping meat in this hot season, led me to make trial of water impregnated with fixed air to preserve it from putrefaction; and I can assure you from repeated trials, that in my own family, and in a neighbour's also, we have been enabled to preserve meat, as perfectly sweet and good to the extent of ten days, as at the first killing, and there seems no doubt it might be preserved much longer. I made use of Mr. Parker's apparatus and directions, but repeated the vitriol and chalk, after four or five hours standing, to the same water; which impregnated it much stronger than one operation could do. With this water our housekeeper washed the meat two or three times a day, and has even recovered some meat that had begun to change. It seems to me too important a fact to be passed over, and that to make it most beneficial to the publick it ought to be in your hands,

who

who will beſt know how moſt effectually to direct it to that purpoſe, if you think any material one may be attained.

I am, Sir,

Your very humble ſervant,

Hartwell, W. LEE.
July 19, 1778.

P. S. It gives no taſte to the meat, and we now conſtantly uſe it to all that comes into the houſe.

NUMBER II.

Extract of a ſecond Letter from Sir WILLIAM LEE *on the ſame Subject, and alſo on the Uſe of ſuch Water in putrid Fevers.*

Sir,

I have to add, in confirmation of what I wrote you, that I gave quantities of the water, ſo impregnated, to ſeveral people in this neighbourhood, and alſo an apparatus, and neceſſary inſtructions, to a perſon in the next adjacent market town, in order to ſpread the uſe thereof as ſpeedily as poſſible; and I have had concurrent teſtimony from all but one ſingle perſon, whoſe ſervants I apprehend had not given it fair play, confirming the efficacy of the method. Particularly, a butcher, who deals pretty largely, aſſures me, he found the greateſt ſucceſs from it, and only objects that the *veal* was a little diſcoloured, though kept perfectly ſweet; and the perſon to whom I gave the apparatus thinks it will prove of great advantage, from the ſucceſs of thoſe to whom ſhe has ſold the

the water. I can further add, partly from my own experience, and from the teſtimony of my houſekeeper and family, that not a morſel of fleſh or fiſh treated in this manner, ſuffered the whole ſeaſon after, except one piece of veal which ſhe purpoſely ſuffered to become *green* in order to try the utmoſt force of the water, for ſhe was ſo pleaſed, and prejudiced with the effects ſhe had ſeen, that I believe ſhe thought it capable of reſtoring any thing, though ever ſo putrid. The event in this caſe was, that it ſo far reſtored it to colour and ſmell that ſhe dreſſed it, as perfectly good; but I muſt own to you, that I found it not eatable, from its exceſſive tenderneſs and very vapid flavour.

What led me to the trial in the firſt inſtance of this remedy was the juſt grounds I had to believe, as I thought, that fixed air had been of conſiderable benefit in a poor family, I had relieved and aſſiſted in a violent putrid fever and ſore throat. The father had it in the moſt violent degree, ſo as to be given over by the apothecary, and was thought not able to live 24 hours; but by vigorouſly perſiſting in Dr. Fordyce's method he recovered, though to all appearance he muſt have ſloughed through the inteſtinal canal. Some weeks after he relapſed as bad again as ever, and the wife and a child at the breaſt began to complain, and had ſome ſpecks in the throat, upon which, after cleanſing the houſe as well as poſſible with vinegar and water, a veſſel of the fermenting ingredients was kept conſtantly at work, in the room where they ſat and lay, the man fumigated his throat with the air, by means of a proper pipe, and returned to the before-mentioned plan of Dr. Fordyce's, the woman and child drank only ſtrong chamomile tea about three times a day, and through the bleſſing of Providence all recovered perfectly, and have had no return ever ſince, now more than two years paſt.

Thus

Thus, Sir, I have given you as full and exact a narrative as poſſible of ſimple facts, from which I doubt not you will draw ſome practical advantage to the publick, more than my ſphere of life will admit, which was my inducement to intrude upon your time; and very happy ſhall I be to receive any commands from you, wherein I can in any ſort render myſelf uſeful by any further inveſtigation of this ſubject, and

am, Sir,

Your obedient humble ſervant,

W. LEE.

NUMBER III.

A Letter from Mr. ADAM WALKER, *Lecturer in Natural Philoſophy, to Dr.* PRIESTLEY, *on the Application of fixed Air to an inflamed Breaſt.*

Hampton Court, 6th October.

Dear Sir,

I have lately ſeen ſuch an effect from a topical application of fixed air, that I cannot deny myſelf the pleaſure of communicating it to you.

My wife lay in about ſix months ago; and as ſhe nurſes her children, was very much diſtreſſed with ſore breaſts: ſhe had the advice of the doctors and good women of the neighbourhood, and I ſhall give their preſcriptions and their effects in the order in which they were applied. The inflammation was at leaſt four inches in diameter.

The firſt was a ſeie cloth of bees wax and mutton ſuet, which ſoftened the inflamed part, but rather added to the inflammation.

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2d, A solution of alum in rum, rubbed on the part with a feather.—This astringent cracked the whole, and encrusted it in such a manner that the pain was intolerable, and the inflammation increased.

3d, The jelly-like matter produced by hot water being poured on quince pippins had no effect.

4th, Powdered lapis calaminaris, dried and encrusted the part; and the inflammation grew still worse.

5th, The oil of egg applied six weeks, only softened, did not stop the increasing inflammation.

6th, Fuller's earth increased the inflammation.

7th, A mucilage of gum arabic in Hungary water, also increased the malady.

8th, Bees wax and oil, did neither good nor harm.

9th, Spermaceti ointment, ditto.

10th, Bread poultices produced many red spots round the nipple, and increased the inflammation.

11th, Camphor ointment, ditto.

These having been applied for the space of four months, and the inflammation growing worse and worse, she was prevailed upon not to persevere in nursing the child. However, as I had often recommended an outward application of fixed air, she would rather try this than wean the child; so I fitted up an apparatus in such a manner that the fixed air discharged from chalk by oil of vitriol, issued from the phial containing these materials through a glass funnel large enough to cover the inflamed part of the breast.

This funnel was held so fast to the breast, that no air could escape, but when by its increase, it pressed too hard upon the breast, a little was let out. She held it to the part about half an hour at a time, twice a day, and from the first application it lost its livid appearance: in four days the child sucked without giving pain; and in ten days the cure was completed, and no return of the inflammation ſince, being upwards of two months, and the child is yet unweaned. I am,

Dear Sir,

Yours, &c.

A. WALKER.

NUMBER IV.

A Letter from Mr. BECKET, *Bookſeller in Briſtol, on the Air extracted from the Water of the Hot-well, and on the Air of that City and the Neighbourhood.*

Briſtol, 20th October, 1778.

Dear Sir,

When I had the pleaſure of ſeeing you laſt, you expreſſed to me your deſire of being informed of the nature and quality of the air, contained in the water of the Hot-well, near this place.—Want of leiſure obliged me to defer making the proper experiments till a few days ago; the reſult of which was as follows:—Having made a quantity of this water acquire a boiling heat in a long curved-neck retort, which was quite filled, and the orifice immerged in water, the air was caught in the upper part of the bend, and all extraneous air entirely excluded. After putting it into the air gage, I firſt of all applied to it your teſt of the nitrous air, by which it appeared to be a very pure common air, ſo far dephlogiſticated, as to take exactly an equal quantity of nitrous air before it increaſed in bulk. I repeated the experiment ſeveral times, varying the

the operation and quantity of air, but with the fame refult. At the fame time I alfo tried the air of other kinds of water. The air of rain water, which had ftood in a ciftern, was not fo pure, and but little different from common air; but that which came from the water of a conftant and good fpring, the refervoir of which is in the ftreet in which I live, was nearly the fame as that of the Hot-well water. I could not difcern any appearance of fixed air in it. After it had ftood two days in the gage-tube, with water, the quantity did not appear diminifhed, nor did it render lime-water turbid. As a farther proof, however, of its being really dephlogifticated, a candle would burn in it with fuperior luftre to common air; and when fired with inflammable air, the explofion was confiderably louder.

I have at times, as opportunity permitted, made frequent ufe of your excellent teft of the purity of common air, by means of nitrous air. I have taken confiderable pains in order to prove its accuracy, by mixing together different kinds of common and noxious air, in different proportions; and have frequently been much pleafed in obferving the correct lengths, which thefe columns of air would occupy in the gage-tube, agreeably to what I apprehended they ought to occupy from a calculation of their proportions.

I have generally found that the air in this city, and the adjacent country, will admit of three parts in eight of nitrous air, before it is faturated. I mean, that if I put five parts of common air into the gage-tube, and add to it three parts of good nitrous air, the whole quantity will diminifh to the original five; after this, as much nitrous air as is put into the tube, the column will appear juft fo much longer. I commonly allow two minutes for its ftanding in the tube after fhaking it a little.

Air which I have had brought to me in a bottle from one of the ſick wards of our infirmary, has appeared to be about one ſixth part noxious; which is nearly the ſame ſtate as that brought from the bottom of a coal-pit in King's Wood, and air from a lead ſmelting-houſe has been a third part noxious.

The air brought from an eminence near this place, called Brandon Hill, has been found to be remarkably different, according to the weather and ſituation of the wind. When the wind blows from the city, the air will not take ſo much of the nitrous air to ſaturate it, as when it blows from the country. But to diſcover the difference, proper attention muſt be had to the ſtate of the air in the room where the experiment is made. I am,

Dear Sir,

Your obliged humble ſervant,

J. B. BECKET.

NUMBER V.

A ſecond Letter from Mr. BECKET *to Dr.* PRIESTLEY, *on the Subject of Air from Sea Water.*

Briſtol, 24th Feb. 1779.

Dear Sir,

I am vexed that I was not able to ſend you any ſea water to Calne. I had ſome of it brought to me, which, being taken up too near the ſhore, was thick, and I ſuppoſed, quite unfit for the purpoſe: it was only a few days ago that I was able to procure any that I could depend on. It was taken up about the middle of Caermarthen Bay; and the perſon who put it in the bottles told me, he cloſed them

them up immediately. I proceeded with it in the ſame manner that I informed you I did before with the Hot-well water, and have juſt now finiſhed the experiment. The reſult was very nearly the ſame as that from the Hot-well water, except that the ſea water air was ſomewhat more pure. The common air here, at this time, is exactly in the ſtate which I have fixed as a ſtandard with reſpect to the nitrous air; I mean, that five parts of common air take 3 parts of nitrous, and the whole appears as 5;—whereas the ſea water took 4.25 of nitrous air before it was brought to its original dimenſions. I am,

Dear Sir,

Very ſincerely,

Yours, &c.

J. B. BECKET.

NUMBER VI.

A Letter from Dr. DOBSON, *of* Liverpool, *to* Dr. PERCIVAL, *of* Mancheſter, *on the Air from Sea-water.*

Liverpool, Jan. 10th, 1779.

I now ſend you, my dear ſir, the reſult of the experiments on marine air, or the air procured from our ſea water, by the heat of boiling water.

Marine air put to the teſt of nitrous air, was found to be one eighth of a meaſure better than common air. The air of Liverpool, tried by the ſame nitrous air, was $\frac{1}{16}$ of a meaſure worſe than common air.—The air contained in the bladders of our ſea-weed, $\frac{1}{4}$ of a meaſure worſe than common air.

That the comparative difference of theſe three may more eaſily and immediately be ſeen, I will ſet them down in the manner I generally do in my experiments.—On my graduated tube, the interval between dephlogiſticated air and perfectly noxious air, is divided into forty-two equal parts, and thus forms a ſcale of forty-two degrees.—On this ſcale, o is fixed at the diviſion which marks good common air.—From o up to dephlogiſticated air, takes twenty two of theſe degrees; and from o down to perfectly noxious air, twenty degrees.

On adding one meaſure of nitrous air to two meaſures of marine air, the mixture was ſo much reduced in bulk, as to ſtand at $2\frac{1}{2}$ degrees above o
Liverpool air, ſtood at 1 degree below o
Pod air, 4 degrees below o

Marine air therefore is $2\frac{1}{2}$ degrees better than good common air.—The air of Liverpool, 1 degree worſe; and pod air, or the air from the bladders of our ſea-weed, 4 degrees worſe. How it is, that the air contained in the bladders of our ſea weed, (which were freſh gathered) ſhould differ from that examined by Dr. Prieſtley, I cannot tell.

The following was the method of procuring the air from ſea water.—A quantity of clear rain water was firſt boiled near four hours, ſo as to be freed from its air.—Into this water, when the heat was ſufficiently abated, was put a bottle containing three gallons of ſea water; and over the mouth of this bottle was inverted a cylindrical glaſs receiver, the mouth of which reſted on the ſhoulder of the bottle.—After four hours, the heat of the boiling water had raiſed about ſix ounce meaſures of air, or ſomething more than $\frac{1}{60}$ of the bulk of the ſea water employed.

I ob-

I obſerved, that the effervefcence, heat, and expanſion were much greater, and the ſubſequent diminution much more rapid, on the mixture of marine air with nitrous air, than on the mixture of common air, or pod air, with nitrous air.—Marine air does not precipitate lime from lime water; and how far it is dephlogiſticated, has been already mentioned.

In making the above experiments I was aſſiſted by Mr. William Rathbone, an ingenious young gentleman of this place,

With affection and eſteem,

I remain,

Your's very ſincerely,

MATTH. DOBSON.

In a letter incloſing this, Dr. Percival ſays, "It will "doubtleſs occur to your recollection that ſea-water near "Liverpool muſt be mixed with impurities by the muddy "freſh water of the river Merſey. The *quercus marinus* "alſo, by growing on ſlimy banks will have its pods filled "with worſe air than thoſe which you obſerved in an "open ſea beach laſt ſummer. The ſeaſon of the year "ſhould likewiſe be adverted to."

NUMBER VII.

A Letter from Mr. MAGELLAN, F. R. S. *to* Dr. PRIESTLEY, *on the Efficacy of* fixed Air *for diſſolving the Stone, and in putrid Fevers, tried in* Holland.

Dear Sir,

Prince Gallitzin, the Ruſſian ambaſſador to the States of Holland, in a letter dated the 17th inſtant informs me of an extraordinary cure of a putrid fever by the internal application of fixed air, according to the method of Dr. Hulme, both in draughts and in clyſters; and I have now before me this caſe written by Dr. Janſſens, an able phyſician of Operhout, near Breda, in the Dutch Brabant. The patient was a married woman, of thirty-two years old, whom he was called to attend on the ninth day by the aſſiſtant phyſician, to be conſulted, in an alarming circumſtance, which the patient was already in, almoſt all covered with exanthems, of a red and livid colour, ſhewing the greateſt tendency to the laſt ſtage of general putrefaction. All her limbs were in a ſtate of ſlow convulſion, and particularly with cold ſweatings. The bark and all other means pointed out by medical art had been properly applied, but without any ſucceſs. Dr. Janſſens availed himſelf of the hints he received in a converſation with Prince Gallitzin on the ſubject, and ordered that the decoction of the bark, till then ineffectual with this patient, ſhould be adminiſtred mixed with the ſalt of tartar, and vitriolic acid, both in draughts and clyſters, relying on the effect of the *fixed air*, which was to be diſengaged within the body of the patient. The ſucceſs fully anſwered his expectations, for in three days time all bad ſymptoms were over, and a perfect recovery was the conſequence of this new treatment. Dr. Janſſens in in this letter to Prince Gallitzin, ſays, that although he confidered himſelf obliged to employ the quinquina (by the apprehenſion of a general putrefaction or gangrena, which he feared in ſo alarming a caſe) nevertheleſs he believed, that the *fixed air* had greatly contributed to this cure.

N. B. Prince Gallitzin in his ſaid letter, ſubmits to farther conſideration, whether the antiſeptic virtues of the bark might not depend chiefly upon the large quantity of *fixed air*, it contains, as he has found by the analyſis of this ſubſtance.

I apprehend the above information will be of ſome ſatisfaction to yourſelf, and to every one who like yourſelf, has at the heart, whatever is good to mankind,

I am with the greateſt regard,

And trueſt affection,

Dear Sir, Your moſt obed. humb. ſervt.

London, 27 February —79. J. H. Magellan.

NUMBER

NUMBER VIII.

A Letter from Dr. INGENHOUSZ, F. R. S. *to* Dr. PRIESTLEY, *on the Effect of a new Species of inflammable Air or Vapour.*

Dear Sir,

As you found, that inflammable air becomes powerfully explosive by being mixed with a certain proportion of dephlogisticated air, I will give you a short account of an expeditious method to procure at pleasure any quantity of an inflammable air with very little trouble and a simple apparatus, which I found out in the beginning of last year, and which afforded me and my friends, to whom I communicated this discovery, some satisfaction. You were one of those, who took delight in seeing the experiment, which I had the pleasure of showing to you.

Mr. Volta contrived some kind of pistols, by which he could throw a leaden bullet to a considerable distance, by loading them with inflammable air mixed with common or dephlogisticated air. The force, with which the bullet was propelled, and the loud report accompanying the explosion made him believe, that this air might perhaps become a substitute for gunpowder.

I was not far from believing, that his expectation was well grounded; but after having considered the matter more maturely, I have altered my opinion, and think now, that the power of inflammable air, though great indeed, will afford very little more than an amusing experiment, to be performed in the apartments of philosophers. I have communicated to you my considerations upon that subject, and therefore will not take up your time in placing them in this lettter, especially as I intend to lay them before the Royal Society.

If

If Mr. Volta's expectation of fubftituting inflammable air for gunpowder had been well grounded, the greateft *defideratum*, I think, would have been to find out an eafy and ready method to procure fuch explofive air in any required quantity, or to carry about fuch air ready made, in a concentrated ftate, fo as to occupy as little fpace as poffible, and to be always in readinefs for immediate ufe.

I have perhaps fulfilled thefe conditions as near as poffible; for all the inflammable air neceffary for a piftol fuch as Mr. Volta contrived, is contained in the fpace of one fingle drop of a liquid. So that a pint bottle may contain as much inflammable air exifting, as it were, in a concentrated ftate, as is required to fire an air piftol many thoufand times.

This liquid is *Vitriolic æther*, the moft volatile of all liquids yet known.

An experiment, which I faw at Amfterdam, in Nov. 1777, fuggefted to me this idea. Mr. Enée, a learned gentleman of that city, fhowed me fome experiments with various inflammable airs: in one, he extracted a very powerful inflammable air from equal quantities of oil of vitriol and fpirit of wine, by applying heat to the phial containing thefe ingredients. One fourteenth of this air mixed with common or dephlogifticated air made a very loud report, when fired by an electrical explofion from a Leyden phial, and propelled a leaden ball with a very great force.

I thought immediately, that the trouble of extracting this air in the way mentioned, might be difpenfed with, if fome drops of good æther were poured into the veffel in which it is to be fired. I propofed to try whether my idea was well founded, as foon as I fhould arrive in London, where I propofed to make fome ftay, to fee my old friends, and to acquire what new knowledge I could in medical and philofophical matters.

Being

Being arrived in this capital in the beginning of Jan. 1778, I immediately ſet about to try the experiment. I poured into a ſtrong glaſs tube ſome drops of æther, and directed an electrical ſpark from a charged vial through it; but to my mortification the inflammable air diſengaged from the æther did not kindle. I repeated the experiment in various ways, as for inſtance throwing into the tube a bit of cotton dipped in æther, &c. but all to no purpoſe. However I was much perſuaded in my own mind that the experiment muſt ſucceed in ſome way or other, that the firſt failing could not diſcourage me; and indeed I ſucceeded once or twice before the end of January, by throwing into the tube a bit of paper dipped in æther. Convinced now that I was right I purſued the experiment; but did not venture to ſhow it to my friends till I had hit upon a method of ſucceeding without fear of failing. I communicated early in the ſpring my having diſcovered a method of producing an inflammable air at pleaſure with a very ſimple apparatus, to Sir John Pringle, Preſident to the Royal Society, to Mr. Nairne and Blunt, and ſome others of my friends, but did not procure this air before my friends in any other method than that I ſaw at Amſterdam. But ſoon after I began to ſhow it to a few perſons, and ſince I have divulged it without ſcruple. I found, that the reaſon why I did not ſucceed in my firſt attempt, was, that I always poured in too great a quantity of æther, by which the inflammable air (or rather inflammable vapour, as it is capable of being abſorbed by water) was not ſufficiently diluted, which is a property common to all inflammable airs.

I find that one ſingle drop of this liquid, poured into an inflammable air piſtol, containing about ten cubic inches, would communicate to the air within it a very ſtrong exploſive force.

The

The moſt expeditious and ſureſt method I hit upon, was to plunge the extremity of a ſmall glaſs tube (whoſe bore was about two lines in diameter) into the æther, till 3 or 4 drops entered into the bore, then to ſhut the upper end of the tube, by applying my finger to it. Thus the little quantity of æther, which has entered the tube, will remain ſuſpended in it, and may be conveyed out of the bottle. I put this tube containing the æther immediately into a ſmall *caoutchouk*, or elaſtic gum bottle; then I withdraw my finger from the upper extremity of the tube; and after having taken the tube out of the elaſtic gum bottle, I thruſt the orifice of this bottle into the barrel of the air piſtol, and after giving it a gentle ſqueeze, I withdraw it, and put a bullet or a cork into the mouth of the barrel of the piſtol, when it is ready to be fired by directing an electrical exploſion from a ſmall Leyden phial through it.

It is to be obſerved, that this inflammable air being heavier than common air will ſettle to the bottom of the piſtol, and thus eaſily miſs catching flame from the ſpark, if the piſtol is not ſhook, before the Leyden phial is applied to it. This air poſſeſſes ſome of the remarkable properties of the other inflammable airs, *viz.* it catches flame only where it is in contact with common air, if the air be unmixed, it will not eaſily inflame; and, if it does, it will burn quietly without exploding. It is unfit for reſpiration, and kills an animal plunged in it almoſt inſtantaneouſly; though it perfumes the common air with an agreeable ſmell, and ſeems far from being hurtful to the lungs in ſuch a diluted ſtate.

If a ſmall quantity of camphor is diſſolved in the æther, the exploſive force ſeems to be rather increaſed. I have alſo tried it by diſſolving a ſmall quantity of phoſphorus of *Kunkel* in it, and found it anſwer very well; but this laſt compoſition ſhould not be poured into the piſtol itſelf, as the phofphoric acid adhering to the inner furface of the piftol, foon attracts a coat of moifture, covering the whole cavity of the piftol, by which it will foon mifs taking fire. Upon the whole, this laft compofition, though very brifk in taking fire, is apt to fail, after the experiment has been repeated fome times, which is occafioned, I fancy, by the moifture it communicates to the piftol.

This inflammable air being much heavier than common air, does not fo eafily efcape out of the piftol as inflammable air extracted from metals by the vitriolic acid, if the orifice of the piftol is kept upright and open.

It requires a ftronger electrical fpark than the other inflammable airs, and can fcarcely be kindled with certainty without a coated phial, which however may be very fmall, fo that one fquare inch of coating will be fufficient. It was well known before, that all fpirituous inflammable liquors have an inflammable atmofphere about them, principally when heated, by which they are fometimes fet on fire, when the flame of a candle is imprudently brought too near them. But I think nobody employed this air, in which æther is decompofed, for the purpofe now mentioned, before I communicated it to my acquaintances.

It feems fomewhat remarkable, that though æther being in a liquid ftate does fo eafily evaporate, that fcarce any glafs ftopper can confine its extreme volatility; yet the air, vapour, or elaftic fluid generated by it, is fo far from being of a fimilar volatility, that it will remain even for hours together in an open glafs, without evaporating or mixing with the common atmofphere, or lofing its inflammable quality, which is to be afcribed to the fpecific gravity of this air being greater than that of common air.

As I make no doubt but this air is the fame that might be extracted from oil of vitriol and fpirit of wine by heat, I will give you the following account of the fpecific gravity of of different inflammable airs compared with common air, with which account I was favoured by Mr. Enée:

A veſſel containing common air to the weight of 138 grains, will contain of inflammable air extracted from iron 25 grains; of air extracted from marſhes, 92 grains; and of that extracted from oil of vitriol and ſpirit of wine, 150 grains. I am,

Dear Sir,

Yours, &c.

London,
1ſt March, 1779:

J. INGENHOUSZ.

NUMBER IX.

Further Experiments on PYROPHORI, *in a Letter to the Reverend Dr.* PRIESTLEY: *By* WILLIAM BEWLY.

In my former paper on the ſubject of *Pyrophori*, printed in the appendix to the 3d volume of your *Obſervations on Air*, I ſuggeſted objections againſt the generally received hypotheſis relating to the accenſion of the various claſſes of *Pyrophori* diſcovered by Homberg and M. du Suvigny; and which may properly enough be diſtinguiſhed by the titles of 1. the *Aluminous*, or that of the Homberg; 2. the *Metallic*, or thoſe made with the three vitriols of iron, copper, zinc; and 3. the *Neutral*, or thoſe compoſed of vitriolated tartar and Glauber's ſalt. The two laſt claſſes were diſcovered by M. du Suvigny, who aſcribed the ſpontaneous accenſion of all the three kinds to the preſence of a highly concentrated vitriolic acid, exiſting in them in an uncombined or nearly diſengaged ſtate, and generating

generating a heat ſufficient to kindle the inflammable ingredient, by eagerly attracting moiſture from the air.

In oppoſition to this theory, in my letter above referred to, I aſſerted that *Pyrophori* of all the above-mentioned claſſes might be prepared, which did not contain any vitriolic acid; and that therefore the cauſe of the accenſion aſſigned by M. du Suvigny could not be the true one. I promiſed likewiſe to take an opportunity of deſcribing the proceſſes on which this aſſertion was founded. Though I have ſince had reaſon to ſuſpect that my general propoſition might perhaps require ſome modification, with reſpect to one of the above-mentioned claſſes of *Pyrophori*; this circumſtance by no means affects my aſſertion reſpecting the inſufficiency of M. du Suvigny's theory. I am ſorry that your work is in ſuch forwardneſs at the preſs, as to allow me time only to relate a few of my experiments on this ſubject, and on others nearly connected with it; particularly thoſe reſpecting the hypotheſis ſuggeſted by myſelf. I ſhall begin with thoſe relating to what I have above called the *Neutral Pyrophorus*, or that which M. du Suvigny prepared by ſubſtituting Glauber's ſalt, or vitriolated tartar, in the room of alum. The experiments immediately following, in which, for the ſake of brevity, I ſhall confine myſelf to the *Pyrophorus* made with vitriolated tartar, will ſhew that the preſence of vitriolic acid is not neceſſary to conſtitute a *Pyrophorus* of this ſpecies; and would alone be ſufficient, from analogy, to render it doubtful whether the other two claſſes owe their accenſion to the agency of that acid.

1. To a quantity of vitriolated tartar, I added more than an equal weight of powdered charcoal, and calcined the mixture a long time, in a red heat, in an open crucible; frequently ſtirring the powder, in order to expel from it as much of the vitriolic acid as poſſible. I have

ſometimes

ſometimes repeated the calcination with freſh charcoal. Nevertheleſs, on treating the ſalt, thus deprived of a conſiderable part of its acid, with charcoal, in a crucible or tobacco pipe (in the manner deſcribed in my former paper) I obſerved no diminution in its quality of producing a *Pyrophorus*.

2. Adding ſucceſſively various and increaſing quantities of fixed alcali to the ſalt treated as above, till the vitriolic acid contained in the mixture might be conſidered nearly as an *evaneſcent* quantity; a pyrophorus was ſtill produced, on calcining it with charcoal as before.

3. I mixed equal parts of *ſalt of tartar*, and vegetable or animal coal, or ſometimes three parts of the former with two of the latter, and calcined them in the uſual manner. This compoſition, on being expoſed to the air generally kindled in the ſpace of half a minute, or a minute. It did not burn with ſo much vivacity as the *vitriolic* pyrophori, as it contained no ſulphur. I ſhall hereafter denominate this the *Alcaline* pyrophorus. It differs in no one circumſtance from M. du Suvigny's *Neutral* pyrophori, except in its *not* containing *that very principle* to which he aſcribes their accenſion.

4. It will perhaps be thought a remarkable circumſtance, on which I cannot now, however, dwell particularly, that the coal of blood, after all its colouring, or *phlogiſtic* matter (as it is called) had been exhauſted, ſo that it would no longer furniſh an atom of Pruſſian blue, appeared to be better adapted than before to the producing a pyrophorus.

5. Leſt it might be ſuſpected that the ſalt of tartar which I employed might accidentally contain vitriolated tartar, or vitriolic acid; I repeated the experiment with tartar calcined by myſelf, as well as with nitre fixed or alcaliſed by deflagration with charcoal, and with iron filings: but in all theſe caſes the event was the ſame. It is rather ſurpriſing that this *alcaline* pyrophorus ſhould not have been diſcovered before; as I have more than

once, ſince I firſt obſerved it, on preparing the *Pruſſian alcali*, ſeen the lower part of it take fire, ſoon after its having been turned out of the crucible, and bruiſed; even when the matter had not been covered with ſand.

For the ſake of thoſe who may be inclined to repeat this experiment, I ſhould obſerve that there is ſomething capricious in the proceſs. It has ſucceeded five or ſix times ſucceſſively, and has ſometimes failed as often; though in both caſes, I uſed a mixture of alcali and charcoal taken out of the ſame phial. I know not yet the circumſtance on which this variety in the reſults depends: though I rather apprehend that a ſtronger heat is neceſſary than when vitriolic acid is contained in the ſaline ingredient.

Expecting ſimilar reſults from diverſifying, in a ſimilar manner, M. du Suvigny's experiments on the *metallic* pyrophori, I ſoon found that none of the three vitriols, heated with charcoal *alone*, in my uſual manner, would produce a pyrophorus. I recollected that he conſtantly added an *alcaline ſalt* to the compoſition; though I believe he no where obſerves that this addition is eſſentially neceſſary to the ſucceſs of the proceſs, as will appear from the following experiments made with the green vitriol.

7. Treating in the uſual manner equal parts of calcined green vitriol and charcoal, the powder did not acquire any of the properties of a pyrophorus. It contained no ſulphur, nor *hepar ſulphuris*. In ſhort, the vitriolic acid ſeemed to have been intirely diſſipated; having no baſe to detain it, when diſlodged from the metallic earth: this laſt not appearing to be adapted, like that of alum, to form a *hepar ſulphuris* with ſulphur; although ſome chemiſts have conſidered the metallic and aluminous earths as being nearly relatead.

8. The charcoal and *calx* of iron, left in the laſt proceſs, were calcined again, together with ſome ſalt of tartar added to the compoſition. A pyrophorus was produced, which, on examination, exhibited indications of

its

its containing a ſcarce perceptible portion of *hepar ſulphuris*, undoubtedly formed in conſequence of ſome ſmall part of the green vitriol having eſcaped decompoſition in the preceding proceſs.

9. Thirty grains of *Crocus Martis aſtringens* were calcined with 15 grains of charcoal and the ſame quantity of ſalt of tartar. This mixture likewiſe burnt ſpontaneouſly; though it contained no *hepar ſulphuris*, or vitriolic acid.

Theſe experiments (§ 7. 8. 9.) aſcertain the truth of my aſſertion with reſpect to this claſs likewiſe of M. du Suvigny's pyrophori. The aluminous only remains. My propoſition, ſo far as relates to it, was founded on the following experiment.

10. I procured the earth of alum by a long and violent calcination; uſing double the quantity of inflammable matter directed by Beaumé. Examining a part of it, I found by the uſual teſts, that it neither contained any ſulphur, hepar ſulphuris, or alum undecompounded. I therefore conſidered it as perfectly pure. It repeatedly furniſhed a pyrophorus, as active as when alum itſelf is employed.

11. I have ſince found, however, that this ſuppoſed pure earth contained a ſmall quantity of vitriolated tartar. Having waſhed the earth, which I had neglected to do before, a pyrophorus was not produced in two or three trials made with it. I have likewiſe failed when I have employed this earth precipitated by an alcali; and yet the quantity of vitriolic acid left in the earth (§ 10.) was very inconſiderable. I am ſorry I have not had an opportunity to inveſtigate the cauſe of this *ſeeming* exception to my general propoſition.

The inſufficiency of M. du Suvigny's theory appears, nevertheleſs, evident from theſe experiments. Even allowing the *vitriolic* pyrophori (or thoſe which contain vitriolic acid) to be kindled merely by their attracting moiſture from the atmoſphere; the earthy or ſaline *hepar ſulphuris*, which is conſtantly formed in the proceſs, muſt be the ingredient that attracts this humidity; and not a ſuppofed glacial vitriolic acid, which, in an *uncombined*, or even *loofely combined* ftate, muft have been foon diffipated in a ftrong red heat; to which, fometimes rifing even to a white heat, I have expofed thefe pyrophori four hours, not indeed without a fenfible lofs of bulk, but with no perceptible diminution of their pyrophoric quality.

12. Some of your experiments contained in this volume feem ftrongly to favour the hypothefis I formerly fuggefted, and to fhew that moifture is not the *fole* caufe, at leaft, of the accenfion of pyrophori; as they are kindled in *dry* nitrous and dephlogifticated air †. I calcined the ingredients of my *alcaline* pyrophorus, feparately, in a ftrong heat; mixing with the alcali fome black lead, iron filings, and other matters not inflammable, to prevent fufion or vitrification. The falt did not grow warm on breathing upon it, and imparted only a flight warmth to my hand previoufly moiftened. *It ftill continued mild.* No warmth was produced on mixing it with the charcoal from the other pipe, or with fome moift charcoal, or fulphur.

13. I muft obferve, however, that on calcining the alcaline falt with twice its weight of the calx of *Pruffian blue* deprived of all its *colouring matter* by repeated digeftions in alcaline lixivia, the refults were fomewhat different. The mixture did not indeed grow warm on breathing upon it, or on mixing the charcoal with it; but it became pretty hot on adding a little water to it. *The alcali was become perfectly cauftic.* It had likewife diffolved a confiderable quantity of the aluminous earth. A folution of it exhibited a kind of *liquor filicum*. A little fpirit of vitriol added precipitated the earth. Neutralifing the liquor, the earth was inftantly rediffolved, and the liquor was ftrongly aluminous: an alcali added again precipitated the earth.

14. In

† See page 64, and 259 of the prefent volume: The inftantaneous accenfion of the pyrophorus in thefe two inftances cannot be afcribed to the fmall portion of phlegm, that can reafonably be fuppofed to be contained in the nitrous and dephlogifticated air, which was thrown up into fmall jars, ufed in thefe experiments.

14. In the *alcaline* pyrophorus, ſome kind of combination ſeems to be formed between the alcali and ſome principle in the coal. On the firſt degree of warmth produced on breathing upon it, a faint phlogiſtic ſmell is perceived. Acids added to a ſolution of it precipitate a ſmall quantity of a ſubſtance that does not ſeem to be the *mere coal* diſſolved by the alcali, but a kind of ſulphur, which, however, does not kindle ſo readily as *vitriolie ſulphur*, and which leaves ſome aſhes, probably the earth of the coal.

In a curious paper on pyrophori, publiſhed by *M. Prouſt*, in the Journal *de Medecine* for July laſt, with a copy of which I have been favoured by the ingenious author; after reciting ſome of my experiments, and concurring with me in rejecting M. du Suvigny's theory, he briefly deſcribes a variety of new pyrophori, which neither contain vitriolic acid; or ſeem likely to owe their accenſion to the attraction of humidity from the air. They principally conſiſt of a coaley matter ſimply divided by metallic or other earths. Such are the ſediment left on the filtre in preparing *Goulard's extract*, various combinations of tartar, or its acid, or the acetous acid, with metals, calcareous earth, *&c.* M. Prouſt aſſerts likewiſe the detonation of charcoal, firſt ignited and ſuffered to cool, with nitrous acid; an experiment which did not formerly ſucceed with me, probably on account of the weakneſs of the acid I employed. It is to be hoped that he will favour us with a more particular detail of his very intereſting experiments, which cannot fail to throw conſiderable light on this ſubject. It may, perhaps, be further illuſtrated by attending to the Abbé Fontana's late curious diſcovery, relative to the ſingular property which charcoal, previouſly heated, poſſeſſes of attracting and abſorbing great quantities of air, while it is cooling.

Great Maſſingham, I am, &c.
March 6, 1779. WM. BEWLY.

Remarks on ſome Parts of this Volume.

HAVING, in ſome parts of this volume, ventured to launch beyond the bounds of the doctrine concerning *air* into the region of a more extenſive chemiſtry, in which I profeſs myſelf to be but a novice, and being unwilling to advance any trite obſervations as diſcoveries of my own, and more eſpecially fearful of having fallen into ſome miſtakes, I begged the favour of my chemical friends, Mr. Bewly, Mr. Keir, and Mr. Hey, to peruſe the work when it was printed off, and to communicate ſuch obſervations as might enable me to make it, in any reſpect, more correct than I was able to do it myſelf. Accordingly they were all ſo obliging as to go over the whole with that view, and the following are the remarks for which my readers and myſelf are indebted to their friendſhip; as well as for ſome of the corrections inſerted in the *errata*.

P. 19. l. 5. "I doubt whether the ſtrength of nitrous "acid can be aſcertained by the quantity of nitrous air "which it produces during a ſolution of copper, as I "think a phlogiſticated acid would produce more air "than an unphlogiſticated acid would, of equal ſtrength. "Perhaps the ſtrength of acids is beſt aſcertained by their "denſity." *Mr. Keir.*

P. 64 and 259. I have called the preparation deſcribed in the Appendix to my third Volume, p. 402, by the name of *Mr. Bewly's pyrophorus.* But the pyrophorus which

is

is properly his is only announced in general terms in that Appendix, and is defcribed at large in the Appendix to the prefent Volume.

P. 86. l. 10. The difcovery of the power of the marine acid to diffolve earthy fubftances has been long known to chemifts, as I have myfelf obferved in the preceding page l. 20. I ought therefore to have expreffed myfelf in fome fuch manner as this, *Having obferved the effect of the folution of earths in the marine acid*, I was, &c.

P. 91. l. 19. "Some of the fubftances here mentioned "are generally believed to be acted upon by the marine "acid, and pretty explicit experiments would be requifite to "prove that, notwithftanding all the proper attention had "been paid to heat, time, pounding of the materials, "different ftrength of the acid, and other ufual circum- "ftances, this acid was incapable of acting upon thefe "fubftances. Zeolyte is faid to be foluble in acids in "general. The alkaline bafis of cream of tartar has been "feparated by the vitriolic and nitrous acids, and it would "be remarkable if it could not be by the marine acid. "Borax is generally faid to be capable of being de- "compofed by all, even the vegetable acid. The action "of acids on none of thefe fubftances is accompanied "with effervefcence." Mr. Keir.

N. B. Obferving no effervefcence, or change of colour in the acid, there was no fuch effect as I was looking for; but I expreffed myfelf too generally in faying there was no fenfible effect at all.

P. 107. Common falt contains a portion of a falt confifting of the marine acid united with an earthy bafis. By boiling, the acid efcapes, and the earthy matter makes the liquor cloudy, and depofits an incruftation. The fame appearance is obferved in boiling down fea water to make falt. Mr. Keir.

P. 122. Section XIV. I by no means meant to insinuate that the convertibility of the volatile vitriolic acid into the common vitriolic acid was a discovery of mine; but only that the *facts* here recited are new proofs of it, or rather facts worth notice independent of that object.

P. 234. l. 5. (b) *Almost all the mercury is* lost. "You "mean only that the mercury continues in the state of a "calx, or a sublimate. But Mr. Bayen revived 4 "drachms and 15 grains from an ounce of turbith mi-"neral, without charcoal or other addition. Rozier tom 6. "part 2. Dec. 1775, p. 490." Mr. Bewly.

P. 392. l. 12. *Common pit-coal yields no fixed air.* "I "remember to have obtained a great deal of fixed air, "mixed with inflammable air, from the pit-coal I tried, "which was that of Stourbridge." Mr. Keir.

P. 404. l. 8. I observe that the cream of tartar appeared, from this experiment, to be of the nature of vegetable matter. Dr. Hales, as Mr. Keir reminds me, obtained from tartar one third of its weight of air, and that, in his own Treatise on Gases, he has observed that the air obtained from it was a mixture of fixed and inflammable air.

P. 413. l. 16. "Mr. Rouelle has shewn that water "impregnated with fixed air does dissolve calxes of iron. "See Lavoisier's Opuscules." Mr. Keir.

P. 419. l. 18. "The heat would as soon decompose "and blacken the oil itself, as it would a straw: for "when straws, &c. are blackened by heat, it is in conse-"sequence of the decomposition of the oil which they "contain." Mr. Keir.

A D-

ADDITIONAL OBSERVATIONS.

1. *Of the Effect of Light on Water.*

MY obfervation that *light* difpofes water, containing calcarious and other fubftances, to make a depofit of a greenifh or brownifh matter, and then to yield dephlogifticated air, feems to be confirmed by the following experiment.

On the 19th of Feb. 1779, I placed two jars of pump water, each containing about 170 ounces, in the fame fouth window, one of them nearly covered from the fun with brown paper, and the other quite uncovered. In about ten days the water in the uncovered jar had yielded about four ounce meafures of air, and the covered jar only a few bubbles. Taking a journey I could make no farther obfervations on thefe jars till my return; but on the fecond of April I found that the uncovered jar had yielded ten ounce meafures of air, fo pure that one meafure of it and one of nitrous air, occupied the fpace of .84 meafures; whereas the covered jar had very little more than one ounce meafure, and with this the meafures of the teft were 1.55 meafures; *i. e.* by no means fo pure as the former. Alfo the uncovered jar had a fediment larger than the other in about the fame proportion, *viz.* of ten to one. Oil of vitriol expelled from this fediment a very great quantity of fixed air. N. B. The loweft part of the jar was not covered with the paper, left being moiftened with the water, in the difh in which the jar ftood inverted, it fhould imbibe the water, and caufe it to evaporate too foon.

2. *Of the Solution of Copper in the Sand Heat.*

The faline fubftance formed by the union of copper and fpirit of nitre is faid to be extremely deliquefcent, but that which is mentioned p. 415, l. 6. I find not to deliquefce at all. As this ſeems to be a new chemical preparation, and is eaſily made, it may perhaps be of ſome uſe, as a cauſtic, or otherwiſe. It may be worth while to examine this ſubſtance, and alſo that from mercury in other reſpects, and to extend the proceſs to other metallic ſubſtances.

3. *Of Sulphur from vitriolic Acid Air.*

The production of real ſulphur from water impregnated with vitriolic acid air may perhaps help to explain the relation that ſulphur bears to water, and decide the diſputes about the preſence of ſulphur in ſome mineral waters.

I would farther obſerve, that the vitriolic acid air with which the water, in the experiment here referred to, was impregnated, was made from a *metal*, viz. copper. This circumſtance Mr. Bewly thinks renders the fact more curious; as affording an additional and ſtriking proof of the ſtrict identity of the inflammable principle in *metals*, and in *oils* and other inflammable ſubſtances.

4. *Of Cures effected by fixed Air.*

I have received from Mr. Magellan a ſecond letter, which I cannot conveniently inſert, containing an account of a *quartan ague* (the conſequence of a bilious complaint) being cured by the uſe of fixed air, in Dr. Hulme's method; and what is more remarkable ſtill, a cure of a *dropſy*, after all other remedies had failed, and the patient had been tapped five times.

The phyſician who made theſe ſucceſsful preſcriptions was Dr. Coopmans of Franeker in Frezeland, and the account was tranſmitted to Mr. Magellan from Prince Gallitzen at the Hague.

The more confiderable ERRATA of the prefs, and other corrections.

N. B. (*b*) means *from the bottom.*

Introduction, p. 29. l. 13. for fig. 3. read fig. 4.
page. 2. l. 3 (*b*) for *more*, read *mere*.
77. l. 13. and p. 246. l. 9. for *hundred* read *hundredth*.
169, l. 2. 3. read *to the mercury from the water*.
176. l. 9. read *mercury in water*.
188. l. 1 (*b*) for *notwithftanding* read *and*.
204. l. 6. for *dephlogifticated* read *phlogifticated*.
205. l. 9. for *it* read *an ounce of it*.
213. l. 3. 4 (*b*) dele *on which the vitriolic acid has no proper action*.
222. l. 6. 8. for *nitre* read *ochre*.
144. l. 6. for *a pound* read *an ounce*.
265. l. 6. (*b*) for *having* read *leaving*.
282. l. 6. read *may be owing, in fome meafure*.
285. l. 1 (*b*) read *was worked a full half hour*.
359. l. 9. read *North Eaft*.
380. l. 5. (*b*) for *phlogifticated* read *phlogifton*.
381. l. 1. (*b*) for *three* read *two*.
404. l. 8. read *that it is of the nature*.
472. l. 16. (*b*) for *exanthems* read *exanthemata*.
479. l. 8. (*b*) for *the Homberg* read *Homberg*.
—— l. 6. for *zinc* read *and zinc*.
480. l. 7. for *agency* read *prefence or agency*.

ERRATA of lefs confequence.

page 9. l. 4. for *did* read *I did*.
15. l. 2. and 367. l. 7. dele *and*.
34. l. 3. for *was* read *is*.
42. l. 8. and 47. l. 5. (*b*) dele *it*.
94. l. 2 (*b*) for *the* read *in the*.
97. l. 8. for *efaped* read *efcaped*.
168. l. 15. for *hypothefis* read *hypothefes*.
187. l. 12. for *had* read *was*.
211. l. 12. for *Jortis* read *Fortis*.
273. l. 4. read *tubes filled with inflammable air*.
280. l. 19. for *ventulated* read *ventilated*.
287. l 10. for *that* read *that the*.
318. l. 19, for *the* read *that the*.
352. l. 3. for *the pan* read *a pan*.
359. l. 1 (*b*) for *air* read *the air*.
382. l. 13. read *inflammable air*.
392. l. 15. for *lay* read *lie*.
400. l. 5 (*b*) read *the fame appearances*.
479. l. 6. (*b*) read *and zinc*.

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